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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

An Estimate of the Philosophical Character of Dr Priestley.

By WILLIAM HENRY, M. D., F. R. S. &c. &c.*

THE principal source of the materials of the following sketch, is the work in which the discoveries of Dr Priestley were originally announced to the public. It consists of six volumes in octavo, which were published by him at intervals between the years 1774 and 1786; the first three under the title of "Experiments and Observations on different kinds of Air;" and the last three under that of "Experiments and Observations relating to various Branches of Natural Philosophy, with a continuation of the Observations on Air." These volumes were afterwards methodized by himself, and compressed into three octavos, which were printed in 1790. As a record of facts, and as a book of reference, the systematized work is to be preferred; but as affording materials for the history of that department of science which Dr Priestley cultivated with such extraordinary success, and, still more, for estimating the value of his discoveries, and adjusting his station as an experimental philosopher, the simple narrative, which he originally gave in the order of time, supplies the amplest and the firmest ground-work.

In every thing that respects the history of this branch of experimental philosophy, the writings and researches of Dr Priestley, to which I have alluded, are peculiarly instructive. They are distinguished by great merits, and by great defects; the latter of which are wholly undisguised by their author. He unveils, with perfect frankness, the whole process of reasoning, which led to his discoveries; he pretends to no more sagacity than belonged to him, and sometimes disclaims even that to which he was fairly entitled; he freely acknowledges his mistakes, and candidly confesses when his success was the result of accident, rather than of judicious anticipation; and by writing historically and analytically, he exhibits the progressive improvement of his views, from their first dawnings to their final and distinct development. Now, with whatever delight we may contemplate a systematic arrangement, the materials of which have been judiciously selected, and from which every thing has been excluded that is not essential to the harmony of the general design, yet there can be no question that, as elucidating the operations of the human mind, and enabling us to trace and appreciate its powers of invention and discovery, the analytic method of writing has decided advantages.

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* Read to the first meeting of the British Association for the Promotion of Science, at York, September 28. 1831. A beautiful Biographical Memoir of Dr Priestley, by Baron Cuvier, is printed in the Number for July 1827 of this Journal.

To estimate, justly, the extent of Dr Priestley's claim to philosophical reputation, it is necessary to take into account the state of our knowledge of gaseous chemistry at the time he began his inquiries. Without underrating what had been already done by Van Helmont, Ray, Hooke, Mayow, Boyle, Hales, Macbride, Black, Cavendish, and some others, Priestley may be safely affirmed to have entered upon a field, which, though not altogether untilled, had yet been very imperfectly prepared to yield the rich harvest, which he afterwards gathered from it. The very implements with which he was to work were for the most part to be invented; and of the merits of those which he did invent, it is a sufficient proof that they continue in use to this day, with no very important modifications. All his contrivances for collecting, transferring, and preserving different kinds of air, and for submitting those airs to the action of solid and liquid substances, were exceedingly simple, beautiful, and effectual. They were chiefly, too, the work of his own hands, or were constructed under his directions by unskilled persons; for the class of ingenious artists, from whom the chemical philosopher now derives such valuable aid, had not then been called into existence by the demands of the science. With a very limited knowledge of the general principles of chemistry, and almost without practice in its most common manipulations;—restricted by a narrow income, and at first with little pecuniary assistance from others;—compelled, too, to devote a large portion of his time to other pressing occupations, he nevertheless surmounted all obstacles; and in the career of discovery outstripped many who had long been exclusively devoted to science, and were richly provided with all appliances and means for its advancement.

It is well known that the accident of living near a public brewery at Leeds, first directed the attention of Dr Priestley to pneumatic chemistry, by casually presenting to his observation the appearances attending the extinction of lighted chips of wood in the gas which floats over fermenting liquors. He remarked, that the smoke formed distinct clouds floating on the surface of the atmosphere of the vessel, and that this mixture of air and smoke, when thrown over the sides of the vat, fell to the ground; from whence he deduced the greater weight of this sort of air than of atmospheric air. He next found that water imbibes the new air, and again abandons it when boiled or frozen. These more obvious properties of fixed air having been ascertained, he extended his inquiries to its other qualities and relations; and was afterwards led by analogy to the discovery of various other gases, and to the investigation of their characteristic properties.

It would be inconsistent with the scope of this essay to give a full catalogue of Dr Priestley's discoveries, or to enumerate more of them than are necessary to a just estimate of his philosophical habits and character. He was the unquestionable author of our first knowledge of oxygen gas, of nitrous oxide, of muriatic, sulphurous, and fluor acid gases, of ammoniacal gas, and of its condensation into a solid form by the acid gases. Hydrogen gas was known before his time; but he greatly extended our acquaintance with its properties. Nitrous gas, barely discovered by Dr Hales, was first investigated by Priestley, and applied by him to eudiometry. To the chemical history of the acids derived from nitre, he contributed a vast accession of original and most valuable facts. He seems to have been quite aware that those acids are essentially gaseous substances, and that they might be exhibited as such, provided a fluid could be found that is incapable of absorbing or acting upon them*. He obtained, and distinctly described†, the curious crystalline compound of sulphuric acid with the vapour of nitrous acid, or, more correctly, of sulphuric and hyponitrous acids, which, being of rare occurrence, was forgotten, and has since been rediscovered, like many other neglected anticipations of the same author. He greatly enlarged our knowledge of the important class of metals, and traced out many of their most interesting relations to oxygen and to acids. He unfolded, and illustrated by simple and beautiful experiments, distinct views of combustion; of the respiration of animals, both of the inferior and higher classes; of the changes produced in organized bodies by putrefaction, and of the causes that accelerate or retard that process; of the importance of azote as the characteristic ingredient of animal substances, obtainable by the action of dilute nitric acid on muscle and tendon; of the functions and economy of living vegetables; and of the relations and subserviency which exist between the animal kingdoms. After trying, without effect, a variety of methods, by which he expected to purify air vitiated by the breathing of animals, he discovered that its purity was restored by the growth of living and healthy vegetables, freely exposed to the solar light.

It is impossible to account for these and a variety of other discoveries, of less importance singly, but forming altogether a tribute to science, greatly exceeding, in richness and extent, that of any contemporary, without pronouncing that their author must have been furnished by nature with intellectual powers far surpassing the common average of human endowments. If we examine with which of its various faculties the mind of Dr Priestley was most eminently gifted, it will, I believe, be found that it was most remarkable for clearness and quickness of apprehension, and for rapidity and extent of association. On these qualities were founded that apparently intuitive perception of analogies, and that happy facility of tracing and pursuing them through all their consequences, which led to several of his most brilliant discoveries. Of these analogies many were just and legitimate, and have stood the test of examination by the clearer light, since reflected upon them from the improved condition of science. But, in other cases, his analogies were fanciful and unfounded, and led him far astray from the path which might have conducted him directly to truth. It is curious, however, as he himself observes, that in missing one thing, of which he was in search, he often found another of greater value. In such cases, his vigilance seldom failed to put him in full possession of the treasure upon which he had stumbled. Finding by experience, how much chance had to do with the success of his investigations, he resolved to multiply experiments, with the view of increasing the numerical probabilities of discovery. We find him confessing, on one occasion, that he "was led on, by a random expectation of some change or other taking place." In other instances, he was influenced by theoretical views of so flimsy a texture, that they were dispersed by the first appeal to experiment. "These mistakes," he observes, "it was in my power to have concealed; but I was determined to show how little mystery there is in the business of experimental philosophy; and with how little sagacity, discoveries, which some persons are pleased to consider great and wonderful, have been made." Candid acknowledgments of this kind were, however, turned against him by persons envious of his growing fame; and it was asserted that *all* his discoveries, when not the fruits of plagiarism, were "lucky guesses," or owing to mere chance*. Such detractors, however, could not have been aware of the great amount of credit that is due to the philosopher, who at once perceives the value of a casual observation, or of an unexpected result; who discriminates what facts are trivial, and what are important; and selects the latter, to guide him through difficult and perplexed mazes of investigation. In the words of D'Alembert, "*Ces hazards ne sont que pour ceux qui jouent bien.*"

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* Series i. vol. ii. p. 175.

† Series ii. vol. i. p. 26.

The talents and qualifications which are here represented as having characterized the mind of Dr Priestley, though not of the rarest kind, or of the highest dignity, were yet such as admirably adapted him for improving chemical science at the time when he lived. What was then wanted, was a wider field of observation ;—an enlarged sphere of chemical phenomena ;—an acquaintance with a far greater number of individual bodies, than were then known ; from the properties of which, and from those of their combinations, tentative approximations to general principles might at first be deduced ; to be confirmed or corrected, enlarged or circumscribed, by future experience. It would have retarded the progress of science, and put off, to a far distant day, that affluence of new facts which Priestley so rapidly accumulated, if he had stopped to investigate, with painful and rigid precision, all the minute circumstances of temperature, of specific gravity, of absolute and relative weights, and of crystalline structure, on which the more exact science of our own times is firmly based, and from which its evidences must henceforward be derived. Nor could such refined investigations have then been carried on with any success, on account of the imperfection of philosophical instruments. It would have been fruitless, also, at that time, to have indulged in speculations respecting the ultimate constitution of bodies ;—speculations that have no solid ground-work, except in a class of facts developed within the last thirty-five years, all tending to establish the laws of combination in definite and in multiple proportions, and to support the still more extensive generalization, which has been reared by the genius of Dalton.

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* These charges, especially that of plagiarism, which had been unjustly advanced by some friends of Dr Higgins, were triumphantly repelled by Dr Priestley, in a pamphlet entitled, "Philosophical Empiricism," published in 1775.

It was, indeed, by the activity of his intellectual faculties, rather than by their reach or vigour, that Dr Priestley was enabled to render such important services to natural science. We should look, in vain, in any thing that he has achieved, for demonstrations of that powerful and sustained attention, which enables the mind to institute close and accurate comparisons ;—to trace resemblances that are far from obvious ;—and to discriminate differences that are recondite and obscure. The analogies which caught his observation lay near the surface, and were eagerly and hastily pursued ; often, indeed, beyond the boundaries within which they ought to have been circumscribed. Quick as his mind was in the perception of resemblances, it appears (probably for that reason) to have been little adapted for those profound and cautious abstractions, which supply the only solid foundations of general laws. In sober, patient, and successful induction, Priestley must yield the palm to many others, who, though far less fertile than himself in new and happy combinations of thought, surpassed him in the use of a searching and rigorous logic; in the art of advancing, by secure steps, from phenomena to general conclusions;—and again in the employment of general axioms as the instruments of farther discoveries.

Among the defects of his philosophical habits, may be remarked, that he frequently pursued an object of inquiry too exclusively, neglecting others, which were necessarily connected with it, and which, if investigated, would have thrown great light on the main research. As an instance, may be mentioned his omitting to examine the relation of gases to water. This relation, of which he had indistinct glimpses, was a source of perpetual embarrassment to him, and led him to imagine changes in the intimate constitution of gases, which were in fact due to nothing more than an interchange of place between the gas in the water and that above the water, or between the former and the external atmosphere. Thus he erroneously supposed that hydrogen gas was transmuted into azotic gas, by remaining long confined by the water of a pneumatic cistern. The same eager direction of his mind to a single object, caused him also to overlook several new substances, which he must necessarily have obtained, and which, by a more watchful care, he might have secured and identified. At a very early period of his inquiries, (viz. before November 1771), he was in possession of oxygen gas from saltpetre, and had remarked its striking effect on the flame of a candle; but he pursued the subject no farther until August 1774, when he again procured the same kind of gas from the red oxide of mercury, and, in a less pure state, from red lead. Placed thus a second time within his grasp, he did not omit to make prize of this, his greatest, discovery. He must, also, have obtained chlorine by the solution of manganese in spirit of salt; but it escaped his notice, because, being received over mercury, the gas was instantly absorbed*. If he had employed a bladder, as Scheele afterwards did, to collect the produce of the same materials, he could not have failed to anticipate the Swedish philosopher, in a discovery not less important than that of oxygen gas. Carbonic oxide early and repeatedly presented itself to his observation, without his being aware of its true distinctions from other kinds of inflammable air; and it was reserved for Mr Cruickshank of Woolwich to unfold its real nature and characters. It is remarkable also, that in various parts of his works, Dr Priestley has stated facts that might have given him a hint of the law, since unfolded by the sagacity of M. Gay-Lussac, " that gaseous substances combine in definite volumes."

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* Series ii. p. 253.

He shows that

1 measure of fixed air unites with $1\frac{6}{7}$ measure of alkaline air,
1 measure of sulphurous acid with 2 measures of do.
1 measure of fluor acid with 2 measures of do.
1 measure of oxygen gas with 2 measures nitrous, very nearly;

and that by the decomposition of 1 vol. of ammonia, 3 vols. of hydrogen are evolved.

Let not, however, failures such as these to reap all that was within his compass, derogate more than their due share from the merits of Dr Priestley; for they may be traced to that very ardour of temperament, which, though to a certain degree a disqualification for close and correct observation, was the vital and sustaining principle of his zealous devotion to the pursuit of scientific truth. Let it be remembered, that philosophers of the loftiest pretensions are chargeable with similar oversights;—that even Kepler and Newton overlooked discoveries, upon the very confines of which they trod, but which they left to confer glory on the names of less illustrious followers.

Of the general correctness of Dr Priestley's experiments, it is but justice to him to speak with decided approbation. In some instances, it must be acknowledged, that his results have been rectified by subsequent inquirers, chiefly as respects quantities and proportions. But of the immense number of new facts originating with him, it is surprising how very few are at variance with recent and correct observations. Even in these few examples, his errors may be traced to causes connected with the actual condition of science at the time; sometimes to the use of impure substances, or to the imperfection of his instruments of research; but never to carelessness of inquiry or negligence of truth. Nor was he more remarkable for the zeal with which he sought satisfactory evidence, than for the fidelity with which he reported it. In no one instance is he chargeable with misstating, or even with straining or colouring, a fact, to suit an hypothesis. And though this praise may, doubtless, be conceded to the great majority of experimental philosophers, yet Dr Priestley was singularly exempt from that disposition to view phenomena through a coloured medium, which sometimes steals imperceptibly over minds of the greatest general probity. This security he owed to his freedom from all undue attachment to hypotheses, and to the facility with which he was accustomed to frame and abandon them ;—a facility resulting not from habit only, but from principle. "Hypotheses" he pronounces, in one place, "to be a cheap commodity ;" in another to be "of no value except as the parents of facts ;" and so far as he was himself concerned, he exhorts his readers "to consider new facts only as discoveries, and to draw conclusions for themselves." The only exception to this general praise is to be found in the pertinacy with which he adhered, to the last, to the Stahlian hypothesis of phlogiston ; and in the anxiety which he evinced to reconcile to it new phenomena, which were considered by almost all other philosophers as proofs of its utter unsoundness. But this anxiety, it must be remembered, was chiefly apparent at a period of life, when most men feel a reluctance to change the principle of arrangement, by which they have been long accustomed to class the multifarious particulars of their knowledge.

In all those feelings and habits that connect the purest morals with the highest philosophy (and that there is such a connection no one can doubt), Dr Priestley is entitled to unqualified esteem and admiration. Attached to science by the most generous motives, he pursued it with an entire disregard to his own peculiar interest. He neither sought, nor accepted when offered, any pecuniary aid in his philosophical pursuits, that did not leave him in possession of the most complete independence of thought and of action. Free from all little jealousies of contemporaries or rivals, he earnestly invited other labourers into the field which he was cultivating; gave publicity, in his own volumes, to their experiments ; and with true candour, was as ready to record the evidence which contradicted, as that which confirmed, his own views and results. Every hint which he had derived from the writings or conversation of others was unreservedly acknowledged. As the best way of accelerating the progress of science, he recommended and practised the early publication of all discoveries; though quite aware that, in his own case, more durable fame would often have resulted from a delayed and more finished performance. "Those persons," he remarks, "are very properly disappointed, who, for the sake of a little more reputation, delay publishing their discoveries till they are anticipated by others."

In perfect consistency with that liberality of temper which has been ascribed to Dr Priestley, it may be remarked also, that he took the most enlarged views of the scope and objects of Natural Science. In various passages of his works he has enforced, with warm and impressive eloquence, the considerations that flow from the contemplation of those arrangements in the natural world, which are not only perfect in themselves, but are essential parts of one grand and harmonious design. He strenuously recommends experimental philosophy as an agreeable relief from employments that excite the feelings or overstrain the attention; and he proposes it to the young, the high-born, and the affluent, as a source of pleasure unalloyed with the anxieties and agitations of public life. He regarded the benefits of its investigations, not merely as issuing in the acquirement of new facts, however striking and valuable; nor yet in the deduction of general principles, however sound and important; but as having a necessary tendency to increase the intellectual power and energy of man, and to exalt human nature to the highest dignity of which it is susceptible. The springs of such inquiries he represents as inexhaustible; and the prospects, that may be gained by successive advances in knowledge, as in themselves "truly sublime and glorious."

Into our estimate of the intellectual character of an individual, the extent and the comprehensiveness of his studies must always enter as an essential element. Of Dr Priestley it may be justly affirmed, that few men have taken a wider range over the vast and diversified field of human knowledge. In devoting, through the greater part of his life, a large portion of his attention to theological pursuits, he fulfilled, what he strongly felt to be his primary duty as a minister of religion. This is not the fit occasion to pronounce an opinion of the fruits of those inquiries, related as they are to topics which still continue to be agitated as matters of earnest controversy. In Ethics, in Metaphysics, in the philosophy of Language and in that of General History, he expatiated largely. He has given particular histories of the Sciences of Electricity and of Optics, characterized by strict impartiality, and by great perspicuity of language and arrangement. Of the mathematics, he appears to have had only a general or elementary knowledge; nor, perhaps, did the original qualities, or acquired habits, of his mind fit him to excel in the exact sciences. On the whole, though Dr Priestley may have been surpassed by many, in vigour of understanding and capacity for profound research, yet it would be difficult to produce an instance of a writer more eminent for the variety and versatility of his talents, or more meritorious for their zealous, unwearied, and productive employment.

APPENDIX.

Since the foregoing pages were written, I have added a few remarks on a passage contained in a recent work of Victor Cousin, in which that writer has committed a material error as to the origin of Dr Priestley's philosophical discoveries. "La chimie," he observes, "est une création du dixhuitième siècle, une création de la France; c'est l'Europe entière qui a appelé chimie Française le mouvement qui a imprimé à cette belle science une impulsion si forte et une direction si sage; c'est à l'exemple et sur les traces de Lavoisier, de Guyton, de Fourcroy, de Berthollet, de Vauquelin, qui se sont formés et que marchent encore les grands chimistes étrangers, ici Priestley et Davy; là Klaproth et Berzelius." (Cours de l'Histoire de la Philosophie, tom i. p. 25.)

It is to be lamented that so enlightened a writer as Victor Cousin, yielding, in this instance, to the seduction of national vanity, should have advanced pretensions in behalf of his countrymen, which have no foundation in truth or justice. Nothing can be more absurd or unprofitable than to claim honours in science, either for individuals or for nations, the title to which may be at once set aside by an appeal to public and authentic records.

It was in England, not in France, that the first decided advances were made in our knowledge of elastic fluids. To say nothing of anterior writers, Dr Black had traced the causticity acquired by alkalies, and by certain earths, to their being freed from combination with fixed air; and Mr Cavendish, in 1766, had enlarged our knowledge of that gas and of inflammable air. In England, the value of these discoveries was fully appreciated; in France, little or no attention was paid to them, till the philosophers of that country were roused by the striking phenomena exhibited by the experiments of Priestley. Lavoisier, it is true, had been led, by an examination of evidence derived from previous writers, to discard the hypothesis of phlogiston. The discovery of oxygen gas by Dr Priestley not only completed the demonstration of its fallacy, but served as the corner-stone of a more sound and consistent theory. By a series of researches executed at great expense, and with consummate skill, the French philosopher verified in some cases, and corrected in others, the results of his predecessors, and added new and important observations of his own. Upon these united, he founded that beautiful system of general laws, chiefly relating to the absorption of oxygen by combustible bodies, and to the constitution of acids, to which alone the epithet of the Antiphlogistic or French theory of chemistry is properly applied. Of the genius manifested in the construction of that system, and the taste apparent in its exposition, it is scarcely possible to speak with too much praise. But it is inverting the order of time to assert, that it had any share in giving origin to the researches of Priestley, which were not only anterior to the French theory, but were carried on under the influence of precisely opposite views. This, too, may be asserted of the discoveries of Scheele, who, at the same period with Dr Priestley, was following, in a distant part of Europe, a scarcely less illustrious career.

It is the natural progress of most generalizations in science, that, at first too hasty and comprehensive, they require to be narrowed as new facts arise. This has happened to the theory of Lavoisier, in consequence of its having been discovered, that combustion is not necessarily accompanied with an absorption of oxygen, and that acids exist independently of oxygen, regarded by him as the general acidifying principle. But after all the deductions that can justly be made on that account from the merits of Lavoisier, he must still hold one of the highest places among those illustrious men, who have advanced chemistry to its present rank among the physical sciences. It is deeply to be lamented that his fame, otherwise unsullied, should have been stained by his want of candour and justice to Dr Priestley, in appropriating to himself the discovery of oxygen gas. This charge, often preferred and never answered, would not have been revived in this place, but for the claim so recently and indiscreetly advanced by M. Victor Cousin. To the credit of Dr Priestley it may be observed, that in asserting his own right, he exercised more forbearance than could reasonably have been expected under such circumstances In an unpublished letter to a friend, he thus alludes to the subject of M. Lavoisier's plagiarism. " He," (M. Lavoisier) " is an *Intendant of the Finances*, and has much public business, but finds leisure for various philosophical pursuits, for which he is exceedingly well qualified. He ought to have acknowledged that my giving him an account of the air I had got from *Mercurius Calcinatus*, and buying a quantity of M. Cadet while I was at Paris, led him to try what air it yielded, which he did presently after I left. I have, however, barely hinted at this in my second volume.*" The communication alluded to was made by Dr Priestley to M. Lavoisier in October 1774; and the memoir, in which the latter assumes to himself the discovery that *mercurius calcinatus* (red oxide of mercury) affords oxygen gas when distilled *per se*, was not read to the Academy of Sciences before April 1775 †. In evincing so little irritability about his own claim, and leaving its vindication with calm and just confidence to posterity, the English philosopher has lost nothing of the honour of that discovery, which is now awarded to him, by men of science of every country, as solely and undividedly his own.

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* Letter to the late Mr Henry, dated Calne, December 31. 1775.

† See an Abstract *of this Memoir in the Journal de Rozier*, Mai 1775.

Account of the Russian Vapour-Bath. By T. S. Traill, M. D. Communicated by the Author.*

The existence in Hamburgh of two establishments where the Russian Vapour-Bath is used, brought to my recollection the descriptions given by Acerbi, and other travellers, of the intense heat and sudden transition to cold, so much relished by the nations of Northern Europe, and raised my curiosity to experience in my own person the effects of this singular species of bathing. I was further induced to take this step from finding myself suddenly oppressed with a violent feverish cold, which raised my pulse considerably above 100°, and rendered me little able to join the public dinner-table in the Apollo Saal.

Accompanied by two friends who wished to make the same experiment, I repaired to the Alexanderbad, which is under the direction of its proprietor, a Jewish physician, who had liberally opened it gratuitously to the members of the Society of *Naturforscher*, then assembled at Hamburgh. We were ushered into a very neat saloon, provided with six couches, beside each of which stood a dressing table, and a convenient apparatus for suspending the clothes of the bather. Here we undressed, and were furnished with long flannel dressing-gowns and warm slippers, after which we were all conducted into a small hot apartment, where we were desired to lay aside our gowns and slippers, and were immediately introduced into the room called the bath, in which the dim light admitted through a single window of three panes, just sufficed to shew us that there were in it two persons, like ourselves *in puris naturalibus*; one of whom was an essential personage, the *operator*, the other a gentleman just finishing the process by a copious affusion of cold water over his body. This sudden introduction into an atmosphere of hot steam was so oppressive, that I was forced to cover my face with my hands, to moderate the painful impression on the lips and nostrils, and was compelled to withdraw my head, as much as possible, from the most heated part of the atmosphere, by sitting down on a low bench which ran along two sides of the bath.

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* Read before the Literary and Philosophical Society of Liverpool.

At first our modesty felt some alarm at our perfect nudity, and that of those around us; but I soon *felt* that it would be absolutely impossible to endure the contact of any sort of covering of our nakedness in a temperature so high; and consoled myself with the reflection, that it was no worse than the promiscuous bathing I had so often practised at the sea-baths of Liverpool; an exposure which, notwithstanding my passion for bathing, was always disagreeable at the commencement of each season; but to which custom had soon rendered me indifferent.

The bath-room is about 15 feet long, by about as much in breadth. It is lined with wood, rendered quite black by constant immersion in hot steam. On two sides it has three tiers of benches, or rude couches, each of which is calculated to hold two persons, with their feet toward each other; so that twelve persons might bathe as the same time. The lowest bench projects farthest into the room; they rise two feet above each other; and each has a wooden pillow at the ends.

In one corner of the farther end of the apartment stands the furnace, which is supplied with fuel from without, and has a thin arch of fire-brick turned over the fire, against which the flame reverberates, until the arch is red hot. Over this arch is built a small brick chamber, the only aperture to which is by a small door about two feet long, and fifteen inches wide, opening nearly to the level of the arch. To increase the heated surface, numerous small earthen jars, or broken pottery, are piled on the arch, and all are kept up to a low red heat. On these, a basin of water is occasionally dashed; and the clouds of steam which instantly issue from the door of the heated chamber, form the source of heat employed to maintain the temperature of the bath.

In the corner opposite to the furnace is a reservoir of cold water, into which the person who manages the bath frequently, during our stay in the bath, plunged to cool his surface; a precaution not unnecessary for an individual who is exposed daily eight hours, stark naked, to a temperature quite oppressive to the uninitiated. Yet this exposure and this alternation cannot be unhealthy; for I never saw a more athletic man than this person, who informed me that he had been constantly engaged in this occupation for sixteen or eighteen months.

The centre of the ceiling of the bath-room is perforated by numerous holes which allow a copious shower-bath of cold water to descend on the head of the bather, when a valve managed by a cord is opened.

Such is the apparatus necessary for a Russian vapour-bath.

After remaining some time in the bath, the first sensations of oppressive heat subsided, and I ascended to the second tier of benches, the wood of which, however, was somewhat cooled by the plentiful affusion of cold water. At each remove this operation is repeated; otherwise the contact of the wood would be insupportable to the skin. It is needless to say, that the perspiration very soon began to run from every pore, not merely as a moist exhalation, but ran off in copious streams. This greatly moderated the sensation of heat.

After lying extended for some time on the second tier of benches, a bucket of cold water was dashed on the upper one, and we removed there; but the heat, so near the ceiling, was fully as oppressive as on first entering; and I found it necessary to allow the air to enter my nose through my fingers. If I inhaled it with the mouth wide open, I felt an oppressive heat in my chest; but by degrees even this degree of heat became supportable; though I never was able to sit upright on the upper bench; so strong was the temperature of the humid atmosphere close to the ceiling.

While we were groping our way from bench to bench, the assistant more than once plunged headlong into his cold bath, to refresh himself ere he commenced on us the next part of his professional occupation.

We were one by one requested to descend to the second tier; and the assistant, grasping in his hand a bundle of birch rods, began assiduously to whip his patients, who lay extended on the bench at full length, from head to heel. This application differs essentially from the well remembered scholastic birch discipline; for the leaves are left on the twigs, and the sensations produced in no way resemble the effect of the instrument employed in English schools to convey a *fundamental* knowledge of Greek and Latin into the *heads* of our youth. In fact, this species of whipping is performed very dexterously, with a sort of brushing motion, from the shoulders downwards; and the application becomes general over the body and limbs, as the bather turns on his wooden couch. The sensations produced by this operation are agreeable, and are very far from producing that excessive redness of the surface described by Acerbi.

The operator now anoints the whole body with a liquid mild soap; and, after again mounting to the upper tier for some time, we descend one by one to the middle of the floor, where a powerful affusion of cold water from the shower-bath in the ceiling removes every vestige of soap. This sudden affusion of cold water is remarkably grateful: it is scarcely possible to describe the effect, which is highly exhilarating and refreshing.

It is usual again to undergo the steaming after the temperature of the bath is increased by the affusion of water on the glowing pottery in the furnace. For this purpose, the operator opens the door above described, and placing us out of the direction of the immediate efflux of the steam, he dashes, in successive jets, a small bucket of water into the furnace. The apartment is instantly filled with clouds of steam, at a high temperature; and when the door of the aperture is closed, we resume our places on the benches, gradually proceeding to the highest, as we become inured to the temperature. From the upper tier we finally descend to have the cold shower-bath repeated; after which we leave the bathing-room, are rubbed dry by assistants in the small heated apartment, where we resume the flannel dressing-gown and slippers, and are reconducted to the saloon, where we find the couches spread with blankets; and we recline for half an hour in a most profuse perspiration, and in a state of luxurious languor, and mental tranquillity.

On a subsequent occasion, I provided myself with the means of ascertaining the temperature of the bathing-room, and noted its effect on the pulse of myself and two other bathers. The heat is generally from 45° to 50° of Reaumur; that is, from 133°.25 to 144°.5 of Fahrenheit. On the occasion referred to, it ranged in the bath, during my stay, from 42° to 46° R., = 126°.5 and 125°.5 F. in the lower part of the bathing-room; but I was unable to examine the temperature near the ceiling, on account of the thick vapour, and the intensity of the temperature, which affected my eyes. This temperature, high as it is, is far short of what Acerbi asserts of the Finnish baths; he says that they reached from 70° to 75° of Celsius, = to 158° to 167 of our scale: but perhaps his thermometers were subject to the influence of the open fire-place in the rude baths of that people; for their furnace consisted of a few loose stones piled into a sort of rude arch, over a fire on the floor of the hut: or perhaps he did not accurately ascertain the temperature; as he never entered the bath but momentarily, for the purpose of placing his thermometer; and I am confirmed in this by observing that the Finnish operator, in his plate, appears dressed in her ordinary clothes, which I should think insupportable in so high a temperature as he assigns.

The effect of the Russian vapour-bath is to accelerate the pulse, which soon regains its natural standard on leaving the bath; and, when I took it in a highly feverish state, I was within an hour after entirely free of fever, and able fully to enjoy the philosophic soirée that evening.

On bathing a second time, I was accompanied by the same two friends: our pulses were before about 74 in a minute. On just coming out of the bath,

Dr Traill's pulse, - - =116
Mr Johnston's do. - - = 88
Mr Palk's do. - - - = 88

A quarter of an hour afterwards, while on the couch, they were as follows:

Dr Traill's pulse, - - =114
Mr Johnston's do. - - = 88
Mr Palk's do. - - - = 88

After being dressed, and sitting in an adjoining coffee-room, perhaps one hour after the bath,

Dr Traill's pulse beat, - - =88
Mr Johnston's do. - - =88
Mr Palk's do. - - - =80

These experiments shew the great difference in the excitability of the heart in different individuals, from exposure to the same heat. My pulse, in my best health, is about 70; since I had the gout it ranges from 74 to 80, but is very easily excited; and I have often found it raised to more than 90 by an *interesting* conversation, or even a cup of strong tea.

The process of the vapour-bath is completed by a plentiful supply of towels, with which we gradually dry the surface, while we are well *rubbed down* by an assistant. We then resumed our dress, and retired to a coffee-room, where there was a plentiful supply of newspapers, and had a cup of good coffee for twopence Sterling. As I have already stated, the baths were free to the *naturforscher;* but I ascertained that the whole expense of the bath and its accompaniments is not more than one marc, or sixteenpence English, and for twopence more the bather is entitled to a cup of coffee, and to read the newspapers in a handsome apartment.

I received from the liberal owner permission to examine his splendid establishment of vapour and shower baths devoted to females.

The vapour-bath resembles that already described, but is much neater.

The variety of shower-baths surprised me. They are of every conceivable form, from the powerful stream to the minute drizzing of water from orifices as fine as a needle, which jet tiny streams of warm or cold water, at the option of the bather, in every possible direction on her person. By means of polished brass arms, curved so as to enclose the body, moveable by universal joints, connected with a cistern, and perforated with innumerable minute holes, a *cross-fire* of jets (if I may be allowed the expression) is kept up on any part of the body. If the bather inclines to sit, a perforated seat is placed on a large flat trough, which collects and carries off the water, jets of water play from the various moveable arms from each side, from above, and from below, so that every part of the surface is bedewed. A general stopcock commands the whole flow of water, while each brazen-rod is under the control of one appropriate to itself. These are at the disposal of the bather; and each trough or bath is surrounded by curtains to skreen the person from the eyes of the assistant.

Similar shower-baths are appropriated to gentlemen. The whole forms one of the most elegant and perfect establishments of the kind I have ever seen, and is a source of emolument to the spirited proprietor.

I inquired anxiously into the medical efficacy of the Russian vapour-bath, and found that in chronic rheumatism, in the stiffness of limbs consequent on gout, and other long continued inflammations, in some cases of palsy, in various cutaneous diseases, it is a most powerful and valuable remedy. While in the establishment I saw an invalid enter, who informed me, that, after severe acute rheumatism, of several months' duration, he was so lame that he had been carried by two persons into the bath; but that, after five or six times undergoing the discipline I have described, he could walk alone as well as I saw him (he had walked, aided by a stick, from his house to the bath), and appeared confident that in a little time he should entirely recover the power and flexibility of his limbs.

From all I could learn in Hamburgh, I am inclined to consider the Russian vapour-bath as a most valuable remedy in some chronic diseases, and regret that we have not a similar establishment in any of our medical charitable institutions.

February 31. 1832.

On the Breeding Spots of Birds. By FREDERICK FABER.*

THE learned editor of the interesting Travels of M. Boie in Norway, considers it as indispensable, for the complete development of the eggs, that they come in contact with the external skin of the bird. This is certainly the case; but I doubt very much if it is the reason of their plucking the feathers off their belly. Some water-birds, as the different species of *Colymbus*, preserve the same dense mass of feathers on their belly during breeding, as at other seasons. Most birds, however, at this period have a much thinner covering on their abdomen than usual, and this is produced, in my opinion, partly by the friction of hatching, partly by the excess of animal warmth which is concentrated in that region. The female of the Iceland grous, and of many wading birds, have the breast and belly nearly quite bare while breeding. But this falling out of the feathers is a consequence of hatching, and belongs to the next period. An entirely different relation takes place among some of the boreal aquatic and wading birds. These pluck off a number of feathers from one or more spots of the belly on the development of the pairing impulse, and before they have laid any eggs, or have begun to hatch. This gives rise to certain naked spots, which I call *Breeding spots.* The utility of this arrangement is various. There is generally so thick a layer of feathers upon the belly of most aquatic birds, that without some process of this kind, the eggs would hardly ever be brought directly in contact with the skin of the mother. In the second place, most aquatic birds have no nest, or other means of furnishing warmth to their eggs, even in the coldest climates. The breeding spots thus form as it were a nest on the body of the parents, as they collect with their bills all the eggs into this artificial cavity, so that they are quite surrounded by the feathers.

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* These observations are taken from Faber's very interesting work on the Habits and Manners of Boreal Birds, of which a translation, now finished, will, we trust, soon be published

The discovery of this peculiar phenomenon in the history of the boreal birds is entirely my own. Only occasionally do we find former writers directing our attention to these breeding spots, but none seem to have recognised their real importance. Being only found in the boreal birds, the discovery was reserved for a naturalist who had an opportunity of spending the summer in their native haunts. Gunnerus remarks of the *Procellaria glacialis*[*], that he had found no such cavity, but that the medical student Martin had observed[†] them to possess a hole under the crop beneath the large feathers, which he thought might perhaps serve for the hatching of eggs. Fabricius remarks, also of this bird[‡], that he had found this hollow; his words are, *Aream deplumem sub abdomine etiam reperi.* M. Boie has observed, in his Travels (p. 192), which were written at the same time with my Prodromus, with respect to the *Lestris parasitica*, that this bird lays only two eggs, and shews that the two parents, which sit alternately, have on both sides of the belly a naked spot, of the size of one of the eggs, and the editor hazards the conjecture that these naked spots may be found in many others of the aquatic and wading tribes.

The true uses of these spots I shall now endeavour to unfold. Birds seldom pluck off their feathers in order to lay them in the nest. Those which are most naked of all during the breeding season, either build no nest, or have no feathers in it. Only the *Anas* and *Sula* tear out their feathers to line their nests. Therefore, we do not find in the nest the feathers which have been taken off the body of the bird. It is necessary that a portion of the great mass which covers the abdomen be removed, in order that the eggs come into immediate contact with the epidermis. This is the first use of the breeding spots. It cannot, however, be their only use, because they are wanting in many of the aquatic birds of the compound monogamy, whose coat of feathers, as just mentioned, is no thinner, as in the *Sula* and *Carbo.* They must, therefore, be intended to envelope and furnish the eggs with warmth.

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[*] Mem. of the *Drontheim Soc.* i. 198.

[†] *Transact. of the Royal Acad.* of Sciences of Sweden for 1759.

[‡] *Fauna Groenlandica, p. 86.*

I have found these breeding spots only in the boreal aquatic birds, and confined to those species which belong to the perfect or compound monogamy. It would be extremely interesting if their existence could be established in the aquatic birds of other zones [*]. They are never found in the genera *Colymbus* and *Podiceps*, which belong to the partial monogamy. They are equally wanting in those simply monogamous, as the *Mergus*, *Anas*, *Anser*, *Cygnus.* But all these birds have the habit of plucking out their feathers for the purpose of lining their nests, which does not exist in those birds which belong to the perfect monogamy, such as the *Phalaropus*, *Uria*, *Alca*, *Mormon*, *Carbo*, *Puffinus*, *Sula*, *Sterna*, *Larus*, *Lestris*, and *Procellaria.* Breeding spots are found in all these genera, with the exception of the *Sula* and *Carbo.*

As both male and female of these species share the labours of hatching, the breeding-spots are found in both sexes, with the remarkable exception, however, of the *Phalaropus*, where they exist only in the male [†]. Among the many hundred individuals of these species which I have examined at the breeding season, I have not seen a single instance of these being wanting, or of their varying in position and number in the individuals of the same species. For they are not a consequence of an unusual deficiency of feathers in these birds, but they follow the most precise rules both in regard to position and number, and furnish a sure specific character of the different boreal aquatic birds.

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[*] Since the above was written, I have had an opportunity of ascertaining the existence of these breeding-spots in the Danish gulls and sea-swallows, during a zoological excursion in the summer of 1824. They exist both in the male and female of the *Larus argentatus*, *L. ridibundus*, *Sterna arctica*, *caspia*, *nigra*, and *minuta.* Their position and number is the same as in the northern individuals of these species. In some wading birds, of both sexes, as the *Charadrius hiaticula* and *albifrons*, I found a spot in the middle of the abdomen, besides a thinner cover of feathers on the breast, which they have in common with most land birds, and the other wading birds, at the breeding season.

[†] M. Holbol has since assured me that, in Greenland, he has not only found the breeding-spots solely in the male of the genus, but that he never *saw a female* at the breeding-place. But I have found both mates together at *the nest in Iceland,* but the male only sitting on the young. Can we infer

Their *number* is only two; in my prodromus (p. 90.) it is indeed stated that the *Larus tridactylus* has from three to four. But I had before me at the time specimens which were only commencing the process of the removal of feathers from the belly; and I do not doubt, but that, as in the other northern gulls, these different patches would have united into a single one in the centre of the abdomen, when it had assumed its finished form. The *Phalaropus*, *Uria grylle* and *alle*, *Alca torda*, *Mormon fratercula*, *Lestris*, have two breeding spots. The *Uria brunnichii* and *troile*, *Puffinus arcticus*, *Sterna arctica*, *Larus tridactylus*, *glaucus*, *marinus*, and the *Procellaria glacialis*, have but one spot. One of the most important distinctions between the *Alca torda* and *Uria troile auctorum* is, that the former has two and the latter but one breeding spot.

In regard to *position*, they are always on the belly, never on the breast; and when one only is present, it is constantly in the middle of the belly; when two exist, they are symmetrically on each side. Their *form* is circular and proportioned to the size and number of the eggs which they have to cover. A central spot is always larger than each of a pair.

Their number occasionally corresponds to that of the eggs, but sometimes there are more eggs than spots, as in the *Larus*; in the *Alca torda*, and *Mormon fratercula*, the spots exceed the eggs in number. When a bird has more eggs than spots, these are generally large, and capable of including more than a single egg. When the spots are more numerous than the eggs, these change their position.

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from the deficiency of the breeding-spots in the *Phalaropus*, a similar defect in the breeding impulse? This genus would then be a solitary exception, of one individual laying the eggs, and another hatching them. It must, however, be observed in general, that we can always infer the breeding impulse to be present when breeding-spots exist, but not *vice versa*; as, for example, neither sex of the *Sula* or *Carbo* has breeding spots, although both hatch.

The *Uria grylle*, *Lestris catarractes*, *pomarina*, and *parasitica*, have two eggs and two breeding spots. The *Uria Brunnichii* and *troile*, *Puffinus arcticus*, and *Procellaria glacialis*, have one egg and one spot. The *Uria alle*, *Alca torda*, and *Mormon fratercula*, have but one egg and two breeding spots.

The *Phalaropus cinereus*, and *Platyrhinchus*, lay four eggs, and have but two breeding spots. The *Sterna arctica*, *Larus glaucus*, *marinus*, and *tridactylus*, have sometimes three, sometimes two eggs, but constantly only one breeding spot.

These spots are not entirely meant to supply the place of a nest; they are, therefore, not invariably in an inverse ratio to the building impulse. Certainly, the species which want these spots, as the *Sula* and *Carbo*, build a nest; most of those, also, which are provided with them build no nest, as the *Phalaropus*, *Uria*, *Alca*, *Morindu*, *Puffinus*, *Sterna*, *Lestris*, *Procellaria;* but the genus *Larus* have breeding spots and build nests.

Their presence is therefore merely a proof of the development of the pairing impulse, but is not to be considered as synonymous with the laying of eggs or hatching. Birds pluck out these feathers before even they have joined their mates, and without being certain of breeding that year. Therefore I have found them in May, in individuals of the *Lestris catarractes*, and *Procellaria glacialis*, which were so far out at sea, and removed from the breeding places of the species, that I had good cause for reckoning these barren birds, which pass the summer without breeding. They also exist in the single individuals of the *Lestris parasitica*, which flock about together.

After hatching is over, these bare spots are very quickly again covered with feathers. All traces of them have disappeared in August and September, when the young of some species are not yet fledged.

Analysis of the Stony Pericarp of the Lithospermum officinale. By Captain CHARLES LE HUNTE. (Communicated by the Author.)

THIS may be considered one of the most remarkable substances in the vegetable kingdom, its properties, mechanical and chemical, are those of a mineral, rather than that of a vegetable. The seeds resemble small, pear-shaped, porcelain beads; they are very hard, difficult to break, and have a high polish. When heated, they at first become black; but they do not shrink, nor does a white heat change their form, in the slightest degree; it destroys, however, their lustre, and renders them, when the vegetable matter has been consumed, whiter than they were originally. Before the blowpipe, small pointed fragments of the pericarp may be partially fused; but this requires a good heat.

To determine the nature and quantity of the earthy constituents, the pericarps were carefully separated from the enclosed seeds, and exposed to the action of dilute muriatic acid; a violent effervescence immediately commenced, which did not entirely cease for upwards of two hours. At the end of twelve hours, the acid liquor was decanted, and the pericarps were well washed. Their appearance was not in the least changed; when dried, at a moderate heat, they still retained their original lustre. The acid liquor was found to contain a great deal of lime, a very little phosphate of lime and oxide of iron, with traces of potash and magnesia, which were separated in the usual manner.

The pericarps were then heated to destroy the vegetable matter, which it is exceedingly difficult to effect; but, notwithstanding the intense heat employed, the form of the fragments was not changed by it, they merely lost their lustre, and became very white. When the vegetable matter was entirely consumed, they were again treated with muriatic acid, and left in a warm place for several hours. The acid took up a very little phosphate of lime and oxide of iron. The matter insoluble in the acid, was fused with carbonate of soda, and found to be pure silica. The analysis was repeated, and the composition of the substance appeared to be very uniform. The following is the result:—

Carbonate of Lime, ...	43.70
Silica, ...	16.5
Vegetable matter, small quantity of phosphate of lime and oxide of iron, with traces of potash and magnesia, .	39.8
	100

The silica appears to form the polished surface of the seeds. As the acid acted upon the vegetable matter of the pericarps, and took up a little phosphate of lime and oxide of iron, that could not be weighed accurately, it was scarcely possible to estimate the quantity of carbonic acid that they contained, from the loss of weight caused by the effervescence. I found it a little greater than it ought to have been, on the supposition that the whole of the lime was in the state of carbonate; and I did not think it necessary to have recourse to a more delicate mode of ascertaining its quantity, for I had no reason to suspect the presence of any other salt of lime, excepting a small quantity of the phosphate. When the dilute muriatic acid was applied to the pericarps, the effervescence was brisk; but they appeared to offer some resistance to its action; and when they were not allowed to remain in contact with it, for at least twelve hours, the silica always contained a little lime. Nitric acid, which acted more powerfully upon the vegetable matter, dissolved the lime speedily, the fragments became very thin, and the siliceous coating alone was left; but, in this case, it was quite impossible to make any calculation for the carbonic acid.

An examination of a larger quantity of these seeds might afford some interesting results; and the whole plant is worthy of attention. I may remark, that they had been collected a year when the analysis was made.

On the Vitality of Toads enclosed in Stone and Wood. By the Rev. W. Buckland, F. R. S., F. L. S., F. G. S., and Professor of Geology and Mineralogy in the University of Oxford. Communicated by the Author.

In the month of November 1825, I commenced the following experiments with a view to explain the frequent discoveries of toads enclosed within blocks of stone and wood, in cavities that are said to have no communication with the external air.

In one large block of coarse oolitic limestone, (the Oxford oolite from the quarries of Heddington) twelve circular cells were prepared, each about one foot deep and five inches in diameter, and having a groove or shoulder at its upper margin fitted to receive a circular plate of glass, and a circular slate to protect the glass; the margin of this double cover was closed round, and rendered impenetrable to air and water by a luting of soft clay. Twelve smaller cells, each six inches deep and five inches in diameter, were made in another block of compact siliceous sandstone, viz. the Pennant Grit of the Coal formation near Bristol; these cells also were covered with similar plates of glass and slate cemented at the edge by clay. The object of the glass covers was to allow the animals to be inspected, without disturbing the clay so as to admit external air or insects into the cell. The limestone is so porous that it is easily permeable by water, and probably also by air; the sandstone is very compact.

On the 26th of November 1825, one live toad was placed in each of the above-mentioned twenty-four cells, and the double cover of glass and slate placed over each of them and cemented down by the luting of clay; the weight of each toad in grains was ascertained and noted by Dr Daubeny and Mr Dillwyn, at the time of their being placed in the cells; that of the smallest was 115 grains, and of the largest 1185 grains. The large and small animals were distributed in equal proportion between the limestone and the sandstone cells.

These blocks of stone were buried together in my garden beneath three feet of earth, and remained unopened until the 10th of December 1826, on which day they were examined. Every toad in the smaller cells of the compact sandstone was dead, and the bodies of most of them so much decayed, that they must have been dead some months. The greater number of those in the larger cells of porous limestone were alive. No. 1, whose weight when immured was 924 grains, now weighed only 698 grains. No. 5, whose weight when immured was 1185 grains, now weighed 1265 grains. The glass cover over this cell was slightly cracked, so that minute insects might have entered; none, however, were discovered in this cell; but in another cell, whose glass was broken, and the animal within it dead, there was a large assemblage of minute insects, and a similar assemblage also on the outside of the glass of a third cell. In the cell No. 9, a toad which, when put in, weighed 988 grains, had increased to 1116 grains, and the glass over it was entire; but as the luting of the cell within which this toad had increased in weight was not particularly examined, it is probable there was some aperture in it, by which small insects found admission. No. 11 *had decreased from* 936 grains to 652 grains.

When they were first examined in December 1826, not only were all the small toads dead, but the larger ones appeared much emaciated, with the two exceptions above mentioned. We have already stated that these probably owed their increased weight to the insects which had found access to the cells and become their food.

The death of every individual of every size in the smaller cells of compact sandstone, appears to have resulted from a deficiency in the supply of air, in consequence of the smallness of the cells, and the impermeable nature of the stone; the larger volume of air originally enclosed in the cells of the limestone, and the porous nature of this stone itself (permeable as it is slowly by water and probably also by air) seems to have favoured the duration of life to the animals enclosed in them without food.

It should be noticed that there is a defect in these experiments, arising from the treatment of the twenty-four toads before they were enclosed in the blocks of stone. They were shut up and buried on the 26th of November, but the greater number of them had been caught more than two months before that time, and had been imprisoned altogether in a cucumber frame placed on common garden earth, where the supply of food to so many individuals was probably scanty, and their confinement unnatural, so that they were in an unhealthy and somewhat meagre state at the time of their imprisonment. We can therefore scarcely argue with certainty from the death of all these individuals within two years, as to the duration of life which might have been maintained had they retired spontaneously and fallen into the torpor of their natural hybernization in good bodily condition.

The results of our experiments amount to this; all the toads both large and small inclosed in sandstone, and the small toads in the limestone also, were dead at the end of thirteen months. Before the expiration of the second year, all the large ones also were dead; these were examined several times during the second year through the glass covers of the cells, but without removing them to admit air; they appeared always awake with their eyes open, and never in a state of torpor, their meagreness increasing at each interval in which they were examined, until at length *they were found* dead; those two, also, which had gained an accession of weight at the end of the first year, and were then carefully closed up again, were emaciated and dead before the expiration of the second year.

At the same time that these toads were enclosed in stone, four other toads of middling size were enclosed in three holes cut for this purpose, on the north side of the trunk of an apple tree; two being placed in the largest cell, and each of the others in a single cell; the cells were nearly circular, about five inches deep and three inches in diameter; they were carefully closed up with a plug of wood, so as to exclude access of insects, and apparently were air-tight; when examined at the end of a year, every one of the toads was dead and their bodies were decayed.

From the fatal result of the experiments made in the small cells cut in the apple tree, and the block of compact sandstone, it seems to follow that toads cannot live a year excluded totally from atmospheric air; and from the experiments in the larger cells within the block of oolite limestone, it seems probable that they cannot survive two years entirely excluded from food; we may therefore conclude, that there is a want of sufficiently minute and accurate observation in those so frequently recorded cases, where toads are said to be found alive within blocks of stone and wood, in cavities that had no communication whatever with the external air. The fact of my two toads having increased in weight at the end of a year, notwithstanding the care that was taken to enclose them perfectly by a luting of clay, shews how very small an aperture will admit minute insects sufficient to maintain life. In the cell No. 5, where the glass was slightly cracked, the communication though small was obvious; but, in the cell No. 9, where the glass cover remained entire, and where it appears certain, from the increased weight of the enclosed animal, that insects must have found admission, we have an example of these minute animals finding their way into a cell, to which great care had been taken to prevent any possibility of access.

Admitting, then, that toads are occasionally found in cavities of wood and stone, with which there is no communication sufficiently large to allow the ingress and egress of the animal enclosed in them, we may, I think, find a solution of such phenomena in the habits of these reptiles, and of the insects which form their food. The first effort of the young toad, as soon as it has left its tadpole state and emerged from the water, is to seek shelter in holes and crevices of rocks and trees. An individual, which, when young, may have thus entered a cavity by some very narrow aperture, would find abundance of food by catching insects, which like itself seek shelter within such cavities, and may soon have increased so much in bulk as to render it impossible to go out again, through the narrow aperture at which it entered. A small hole of this kind is very likely to be overlooked by common workmen, who are the only people whose operations on stone and wood disclose cavities in the interior of such substances. In the case of toads, snakes, and lizards, that occasionally issue from stones that are broken in a quarry, or in sinking wells, and sometimes even from strata of coal at the bottom of a coal mine, the evidence is never perfect to shew that the reptiles were entirely enclosed in a solid rock; no examination is ever made until the reptile is first discovered by the breaking of the mass in which it was contained, and then it is too late to ascertain without carefully replacing every fragment (and in no case that I have seen reported has this ever been done) whether or not there was any hole or crevice by which the animal may have entered the cavity from which it was extracted. Without previous examination it is almost impossible to prove that there was no such communication. In the case of rocks near the surface of the earth, and in stone quarries, reptiles find ready admission to holes and fissures. We have a notorious example of this kind in the lizard found in a chalk pit, and brought alive to the late Dr Clarke. In the case also of wells and coal pits, a reptile that had fallen down the well or shaft, and survived its fall, would seek its natural retreat in the first hole or crevice it could find, and the miner dislodging it from this cavity to which his previous attention had not been called, might in ignorance conclude that the animal was coeval with the stone from which he had extracted it.

It remains only to consider the case, (of which I know not any authenticated example), of toads that have been said to be found in cavities within blocks of limestone to which, on careful examination, no access whatever could be discovered, and where the animal was absolutely and entirely closed up with stone. Should any such case ever have existed, it is probable that the communication between this cavity and the external surface had been closed up by stalactitic incrustation after the animal had become too large to make its escape. A similar explanation may be offered of the much more probable case of a live toad being entirely surrounded with solid wood. In each case the animal would have continued to increase in bulk so long as the smallest aperture remained by which air and insects could find admission; it would probably become torpid as soon as this aperture was entirely closed by the accumulation of stalactite or the growth of wood; but it still remains to be ascertained how long this state of torpor may continue under total exclusion from food, and from external air: and although the experiments above recorded shew that life did not extend two years in the case of any one of the individuals which formed the subjects of them, yet, for reasons which have been specified, they are not decisive to shew that a state of torpor, or suspended animation, may not be endured for a much longer time by toads that are healthy and well fed up, to the moment when they are finally cut off from food, and from all direct access to atmospheric air.

The common experiment of burying a toad in a flower-pot covered with a tile, is of no value, unless the cover be carefully luted to the pot, and the hole at the bottom of the pot also closed, so as to exclude all possible access of air, earthworms and insects. I have heard of two or three experiments of this kind, in which these precautions have not been taken, and in which, at the end of a year, the toads have been found alive and well.

Besides the toads enclosed in stone and wood, four others were placed each in a small basin of plaster of Paris, four inches deep and five inches in diameter, having a cover of the same material carefully luted round with clay; these were buried at the same time and in the same place with the blocks of stone, and on being examined at the same time with them in December, 1826, two of the toads were dead, the other two alive, but much emaciated. We can only collect from this experiment, that a thin plate of plaster of Paris is permeable to air in a sufficient degree to maintain the life of a toad for thirteen months.

In the 19th Vol. No. 1, p. 167, of Silliman's American Journal of Science and Arts, David Thomas, Esq. has published some observations on frogs and toads in stone and solid earth, enumerating several authentic and well attested cases; these, however, amount to no more than a repetition of the facts so often stated and admitted to be true, viz. that torpid reptiles occur in cavities of stone, and at the depth of many feet in soil and earth; but, they state not any thing to disprove the possibility of a small aperture, by which these cavities may have had communication with the external surface, and insects have been admitted.

The attention of the discoverer is always directed more to the toad than to the minutiæ of the state of the cavity in which it was contained.

In the Literary Gazette of March 12. 1831, p. 169, there is a very interesting account of the habits of a tame male toad, that was domesticated and carefully observed during almost two years by Mr F. C. Husenbeth. During two winters, from November to March, he ate no food, though he did not become torpid, but grew thin and moved much less than at other times. During the winter of 1828, he gradually lost his appetite and gradually recovered it. He was well fed during two summers, and after the end of the second winter, on the 29th of March, 1829, he was found dead. His death was apparently caused by an unusually long continuance of severe weather, which seemed to exhaust him before his natural appetite returned. He could not have died from starvation, for the day before his death he refused a lively fly.

Dr Townson also, in his tracts on Natural History, (London 1799), records a series of observations which he made on tame frogs, and also on some toads; these were directed chiefly to the very absorbent power of the skin of these reptiles, and show that they take in and reject liquids, through their skin alone, by a rapid process of absorption and evaporation,—a frog absorbing sometimes in half an hour as much as half its own weight, and in a few hours the whole of its own weight of water, and nearly as rapidly giving it off when placed in any position that is warm and removed from moisture. Dr T. contends that as the frog tribe never drink water, this fluid must be supplied by means of absorption through the skin. Both frogs and toads have a large bladder, which is often found full of water; "whatever this fluid may be, (he says), it is as pure as distilled water and equally tasteless; this I assert as well of that of the toad which I have often tasted, as that of frogs."

On the Chemical Constitution of Harmotome, or Cross-stone.

By Arthur Connell, Esq., F.R.S.E. Communicated by the Author *.

In examining some specimens of harmotome from Strontian, I observed some crystals which appeared to present a very different aspect from the ordinary harmotome with which they were associated. The usual crystals of this mineral from the above locality are well known to be of considerable size, and to exhibit the form of a rectangular prism, Plate I. Fig. 1, terminated by a pyramid, the faces of which are set on the lateral edges of the prism, two opposite edges of the pyramid being also replaced by planes. On the other hand, the crystals to which I have referred were a great deal smaller in size, usually not exceeding one-tenth of an inch in length, and of greater transparency; and they presented the apparently very dissimilar form of a rhombic prism, Fig. 3, of considerable acuteness, having the acute angle more or less truncated by the face A, and terminated by a pyramid C, the faces of which were set on the lateral planes of the prism, and its apex truncated. They were spread over the surface of calcareous spar in considerable numbers, and were usually attached to the matrix by one of the extremities D′D. Farther consideration, however, showed that this latter form was in reality merely a modification, although undoubtedly a very considerable one, of the old form, and arose principally from the vertical contraction of the crystal, Fig. 1, and its horizontal elongation in the direction of the faces BB′, the inclination of the several faces to one another remaining always the same; as will be evident by comparing Figures 1, 2, and 3, Fig. 2 representing another form of the crystal, which may be regarded as intermediate between Figs. 1 and 3, and the whole three being placed in parallel position, with their corresponding faces marked by the same letters. In some rare instances, the pyramids C′ C, and the face A, Fig. 3, almost entirely disappear, so that the crystal appears nearly as a simple rhombic prism; and in others equally rare, the face D almost disappears, so as to leave the pyramid nearly without truncation.

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* Read to the Royal Society of Edinburgh, 2d April 1832.

A few minor modifications of both forms also occur, which it is unnecessary to notice.

I was farther confirmed in these views of the connexion between the two forms, by the opportunity which the liberality of Mr Allan afforded me of consulting the interesting catalogue of his collection, drawn up by Mr Haidinger, in which I found a series of figures of harmotome crystals, presenting, in so far as I could judge, a transition of the one form into the other, with some of those lesser modifications to which I have alluded.

As the angle of the faces B, replacing the opposite edges of the pyramid in Fig. 1, has been stated by Mr Phillips as 110° 26′, this will of course become the measure of the rhombic prism, Fig. 3, if the foregoing views of the relation between the two forms are correct.

Before the connexion of the two forms Figs. 1 and 3 had occurred to me, which was not until I had observed a crystal of the form Fig. 2, I commenced an analysis of a portion of the crystals of the rhombic form, Fig. 3, under the idea that they might present some modification of the usual constitution of this mineral; and although they proved to be merely a barytic harmotome, yet the analysis seems to throw some little additional light on the connexion between the barytic and lime varieties of the mineral. The steps of the analysis were as follows.

(*a.*) 7.37 grains of the crystals in coarse powder lost, by ignition, 1.1 grain, equivalent to a loss of 14.925 per cent.

(*b.*) 16.07 grains of the crystals which had been previously treated with acidulated water, to remove all adhering calcareous spar, were reduced to impalpable powder, and then left for three or four days in contact with muriatic acid, a moderate heat being occasionally applied. The mass did not gelatinize; but, as will afterwards appear, the action was quite sufficient for the purpose of analysis. The whole was evaporated to dryness. A little muriatic acid was then poured over it, and left for some hours, when water was added, and heat applied. The silica was then separated by filtration. After ignition, it weighed 7.55 grains. It dissolved in boiling caustic potash ley, except a residue of .47, which was resolved by fusion with carbonated alkalies, and other necessary steps, into .4 of silica, and .01 of oxide of iron. The total silica thus amounts to 7.48 grains.

(*c.*) The liquid from which the silica had been separated was precipitated by ammonia. The precipitate, after being collected on a filter and duly washed, was dried and ignited. It then weighed 2.53 grains. It was dissolved in muriatic acid, and left a residue of .03 of silica. The muriatic solution was then boiled with caustic potash. What remained undissolved by the potash was collected and washed, and then treated with muriatic acid. A residue of .02 of silica was left by the acid. The muriatic solution was boiled with nitric acid, neutralized by ammonia, and precipitated by benzoate of ammonia. The benzoate of iron was burned with a little nitric acid, and the peroxide of iron thus got weighed .03. The residual fluid boiled with carbonate of potash gave an insignificant white precipitate, too small to weigh or examine. There thus remained of alumina, dissolved by the caustic potash, 2.45 grains.

(*d.*) The liquid which had been precipitated by ammonia, together with the washings of the precipitate concentrated by evaporation, was heated, and carbonate of ammonia added to it whilst hot. The precipitate which fell, weighed, after being washed and ignited, 4.36 grains. It was dissolved in dilute muriatic acid, and left .01 of silica. The solution by evaporation afforded tabular crystals of muriate of baryta.

(*e.*) The crystals of muriate of baryta were washed with alcohol. The alcohol was separated, mixed with water, and evaporated to dryness, when a little deliquescent matter was left. This was re-dissolved in water, and the solution precipitated by oxalate of ammonia. The precipitate by calcination afforded .03 of carbonate of lime, equivalent to .0168 of lime. By subtracting from the amount of the precipitate by carbonate of ammonia, the substances afterwards separated from it, we get 4.32 of carbonate of baryta, equivalent to 3.3517 of baryta.

(*f.*) The liquid which had been precipitated by carbonate of ammonia, in (*d*) was evaporated to dryness, and the ammoniacal salt driven off by heat. The residue, after ignition, weighed .5. Dissolved in water it left .02 of silica, giving .48 for the soluble residue. The solution by evaporation gave cubical crystals. Re-dissolved, the liquid was plentifully precipitated by muriate of platinum, which was added in some excess. The whole was then evaporated to dryness at a gentle heat; the dry mass digested with alcohol; the alcoholic solution mixed with water and sulphate of ammonia, and evaporated to dryness. The residue was then ignited, and treated with hot water. The solution by evaporation gave efflorescent crystals of sulphate of soda.

(*g*.) As in this analysis the relative quantities of potash and soda were not determined, a new analysis was undertaken for that purpose. 13.55 grains of harmotome crystals of the same form were decomposed by muriatic acid, as before, and after evaporating to dryness and re-dissolving, the whole earthy contents of the mineral were thrown down at once by carbonate of ammonia. The residual chlorides obtained, as in the preceding process, weighed .38. Their solution was precipitated by muriate of platinum, and the liquid left to spontaneous evaporation. Well characterised prismatic crystals of the double chloride of platinum and sodium were formed. These were taken up by digestion in alcohol. The residual chloride of platinum and potassium, after being well washed with alcohol, and carefully dried, weighed .58, equivalent to .17726 of chloride of potassium. By subtraction we get .20274 for the chloride of sodium. On distributing, in the same proportions, the .48 of soluble residue of the first analysis, we get .2239 of chloride of potassium, and .2561 of chloride of sodium, equivalent to .14163 of potash, and .13647 of soda. I prefer taking the total soluble residue of the first analysis to that of the second, because the crystals analyzed were better formed, and of greater purity.

We thus have in 16.07 grains of the mineral, exclusive of water

Silica, *(b) (c) (d) (f)*	7.56
Alumina, *(c)*	2.45
Baryta, *(e)*	3.3517
Lime, *(e)*	.0168
Potash, *(g)*	.1416
Soda, *(g)*	.1364
Peroxide of Iron,	.03
	13.6865

And, in 100 parts,

Silica,	47.04
Alumina,	15.24
Baryta,	20.85
Lime,	.10
Potash,	.88
Soda,	.84
Peroxide of Iron,	.24
Water,	14.92
	100.11

After I had ascertained the existence of alkalies in the crystals of the new form, I submitted to chemical examination by a similar process a portion of the barytic harmotome of Strontian of the ordinary form, and obtained cubical crystals, the solution of which, when mixed with muriate of platinum, and evaporated at a gentle heat, afforded on re-solution a small quantity of minute yellow scales; and by subsequent spontaneous evaporation, crystals of the chloride of sodium and platinum were formed. I then examined some small twin crystals of barytic harmotome from Andreasberg, in the Hartz, which was, I believe, the locality of the specimen analyzed by Klaproth, and obtained a like result. It seems, therefore, extremely probable, that it will be found universally, that barytic harmotome contains small quantities of potash and soda[*].

The existence of potash and lime in the barytic harmotome of strontian, appears to afford an additional link of connexion between the two varieties into which harmotome has been divided by foreign chemists, baryta in the one being supposed to be replaced by lime and potash in the other. The constitutions of the two varieties have not yet, however, been accurately reconciled to one another. The Berzelian formula of $KS^2 + 2\,CS^2 + 10\,AS^2 + 15\,Aq$ correctly represents the composition of the lime harmotome, according to the analyses of Gmelin and

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[*] I am not aware that potash has before been observed in a proper barytic harmotome. After I had detected the two alkalies in the strontian harmotome, I observed in Berzelius' Jahres Bericht, 6th year, p. 224, an allusion to some unfinished researches of L. Gmelin, showing the presence of soda in a barytic harmotome, but I have never seen any further account of these researches.

Wernekinck. But when we substitute baryta for lime and potash, the formula will not exactly apply to any of the analyses of barytic harmotome with which I am acquainted, although it does not deviate very much from some of them. We cannot, however, I think, draw any argument against the application of the doctrine of replacement to these two varieties, from this want of perfect conformity; because the different analyses of barytic harmotome, scarcely vary more from the formula than they do from one another, and we might as well argue that the different specimens of barytic harmotome which were the subjects of these analyses, were not the same mineral, as deny the occurrence of replacement between the two varieties, on the mere ground of this disconformity. On the other hand, the almost perfect identity between the forms of the two varieties, the approximation between their atomic constitution on substituting one set of oxides for another, and the occurrence of small quantities of potash and lime in the barytic harmotome of Strontian, and of a little baryta in the lime and potash harmotome of Annaröde*, appear all to render it extremely probable that the two will ultimately be found to admit of the application of the principles of isomorphism, or at least of plesiomorphism. We may hope that future and more extended analyses will yet establish the perfect conformity of the constitution of barytic harmotome with that of the other variety, on making the requisite substitution. If this shall be the case, we can hardly doubt that the mineral, which has been in this country called Phillipsite, ought also to be considered as a lime harmotome. The general form of its crystals, as described by Mr Levi †, is the same as that of barytic harmotome; and the measurements of its angles do not present greater discrepancies than between the carbonates of lime, iron, manganese, and magnesia, substances which, if the doctrine of replacement has any foundation at all, must be viewed as composed of plesiomorphous bodies. Neither would it seem that the variations of its cleavage can present any serious obstacle; for cleavage appears chiefly of importance as being indicative of crystalline form; and as long as the cleavage of two minerals affords forms which are identical with some of the crystalline forms which are common to both minerals,—and this is the case in regard to philipsite and harmotome,—we can hardly maintain the diversity of the two substances, merely because these cleavage-forms may not be identical with one another, if other circumstances tend to establish the connexion of the two bodies.

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* See Wernekinck's analysis, afterwards given.

† Annals of Philosophy, November 1825.

I shall here, with the view of enabling every one to draw his own conclusion as to the probability of the ultimate reconciliation of the constitution of the two varieties, subjoin Tables containing all the analyses of the mineral with which I am acquainted, and shall annex the composition of both varieties calculated by the atomic weights of Berzelius, according to the chemical formulæ corresponding with $KS^2 + 2\,CS^2 + 10\,AS^2 + 15\,Aq$ for the lime variety, and $3\,BS^2 + 10\,AS^2 + 15\,Aq$ for the barytic variety. It will be observed that the Strontian mineral approaches nearer the theoretical composition than any of the other barytic varieties, in so far as respects baryta and the other replacing constituents, which is perhaps a step not altogether without importance, towards a more perfect accommodation of the two varieties, although, as respects silica and alumina, some of the other analyses come nearer the formula.

LIME HARMOTOME.

	L. Gmelin.	L. Gmelin.	Wernekinck.	Formula.
Silica, . . .	48.51	48.02	48.36	49.381
Alumina, . .	21.76	22.61	20.20	21.125
Lime, . . .	6.26	6.56	5.91	7.025
Potash, . . .	6.33	7.50	6.41	5.820
Baryta, . . .			.46	
Total Alkalies and Alkaline Earths, .	12.59	14.06	12.78	12.825
Peroxide of Iron,	.99	.18	.41	
Water, . . .	17.23	16.76	17.09	16.647
Total, . .	100.38	100.62	98.64	99.995
Locality, . .	Marburg.	Marburg.	Annaröde near Giessen.	

BARYTIC HARMOTOME.

	Klaproth.	Tassaert.	Wernekinck.	Connell.	Formula.
Silica, . . .	49	47.	44.79	47.04	42.764
Alumina, . .	16	19.5	19.28	15.24	18.294
Baryta, . . .	18	16	17.59	20.85	24.527
Lime, . . .			1.08	.10	
Potash, . . .				.88	
Soda, . . .				.84	
Total Alkaline Earths and Alkalies, .	18	16.	18.67	22.67	24.527
Peroxide of Iron,			.85	.24	
Water, . . .	15	13.5	15.32	14.92	14.415
Total, .	98	96.5	98.91	100.11	99.998
Localities, .	Hartz.	Oberstein.	Sheffenberg, near Giessen.	Strontian.	

I may take this opportunity of mentioning, that after I had detected alkalies in barytic harmotome, it occurred to me to examine Brewsterite again for alkalies by the same process, which was applied to the former mineral; my previous researches for alkalies in Brewsterite having been made by decomposing it by carbonate of baryta, and throwing down the baryta by carbonate of ammonia, a method which renders necessary the ultimate expulsion by heat of a very large quantity of ammoniacal salt, which is apt to carry along with it small quantities of fixed chlorides. I accordingly treated a small quantity of powdered Brewsterite with muriatic acid, leaving them in contact for some days, and occasionally applying heat. By the process already detailed, I ultimately obtained a minute quantity of cubical crystals; and, on examining those by muriate of platinum, they seemed to be entirely chloride of sodium, at least operating with the small quantity of materials which I used, I could not detect potash. The minute quantity of soda is of course in addition to strontia, baryta, and the other constituents which I formerly mentioned in Brewsterite. When my time permits, I intend to execute another analysis of the mineral, to ascertain the exact proportion of alkali it contains.

Remarks on some of Baron Cuvier's Lectures on the History of the Natural Sciences, in reference to the Scientific Knowledge of the Egyptians; of the source from whence Moses derived his Cosmogony, and the general agreement of that Cosmogony with Modern Geology *.

In some of the Numbers of the Edinburgh New Philosophical Journal published in 1830, are given Reports of Lectures on the History of the Natural Sciences by Baron Cuvier; and in pages 342, No. XVI., we find in them the following statement respecting the Hebrew legislator:—" His books shew us, that he had very perfect ideas respecting several of the highest questions of natural philosophy. His cosmogony especially, considered in a purely scientific view, is extremely remarkable, inasmuch as the order which it assigns to the different epochs of creation, is precisely the same as that which has been deduced from geological considerations." This, then, is the issue, in the opinion of Baron Cuvier, of that science, which has been held by many persons to teach conclusions at variance with the Book of Genesis,—when at last more matured by a series of careful observations and legitimate induction, it teaches us precisely what Moses had taught more than three thousand years ago.

But at the same time that the Baron makes this statement, it is implied by him in the accompanying sentences, that the Hebrew legislator had acquired his knowledge of the cosmogony from the Egyptians; for he says, " The leaders of the colonies which issued from Egypt possessed, in general, but a small part of the knowledge of which the privileged caste (the priests) was the depositary. They carried with them only the practical results. The case was different with the Hebrew legislator.

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* " No opinion can be heretical but that which is not true. Truths can never war against each other. I affirm, therefore, that we have nothing to fear from the results of our inquiries, provided they be followed in the laborious but secure road of honest induction. In this way, we may rest assured, we shall never arrive at conclusions opposed to any truth, either physical or moral, from whatsoever source that truth may be derived; nay, rather that new discoveries will ever lend support and illustration to things which are already known, by giving us a larger insight into the universal harmonies of Nature."—*Professor Sedgwick's Address to the Geological Society, February* 19. 1830.

He had been brought up by the Egyptian priests, and knew not only their arts, but also their philosophical doctrines."

In attempting to discuss the merits of the opinion here implied, we would speak in terms of high respect of the illustrious individual who has promulgated it; for such respect is due to one who, without question, has, in the field of natural science, erected a nobler monument to his own fame than any other who has appeared since the days of Newton.

The premises from which it is inferred that the Egyptian priests may have possessed such a knowledge of geology as would furnish a foundation for the cosmogony of Moses, are by much too meagre to warrant such a conclusion. The chief of them is indeed found in what Herodotus states regarding the land of the Delta, by depositions from the waters of the Nile. It is said in page 340, " The Egyptians had very correct ideas on several points in geology; they had well observed the laws of alluvial deposition, and at the present day we account for the formation of the Delta in no other manner than that in which it was accounted for in the days of Herodotus."

In turning to Herodotus, respecting whom many modern discoveries have proved that he was a faithful chronicler of what he saw, although often absurdly credulous of the reports of others, we find no proof in his relation, that the Egyptians had well observed the *laws* of alluvial deposition. With respect to the priests, he states, in the passage referred to by the Baron, only that they informed him of two facts; one, that the greater part of a country, of which he describes the limits, was an addition of land to the Egyptians by the depositions of the Nile; the other, that in the reign of Myris, about nine hundred years before the time of the historian, the land was so low, that if the river rose to the height of eight cubits, it was sufficiently watered; whereas at the time he visited Egypt, unless the river rose fifteen or sixteen cubits the land was not sufficiently watered. This is not science, but history. No reasoning of the priests is added with regard to these simple facts; and the evidence appears conclusive, that he had heard no reasoning of theirs, in this circumstance, that he himself proceeds to reason regarding them with considerable ingenuity, and to prove their high probability from a variety of considerations, and could scarcely have omitted the arguments of the priests had he heard any from them. As far as regards the science contained in this passage of Herodotus, it is perhaps as truly philosophical as any other to be found in his writings; but the philosophy is exclusively that of the Greek himself. The facts, which rested on the authority of the priests, were of a character that it required no science or cultivated understanding to ascertain; no more, indeed, than it requires in the present inhabitants of Cairo to discern when the Nile rises sufficiently to secure a productive crop.

But we are informed also, page 340, that "the properties of minerals were tolerably well examined. The country offered every facility for this; the mountains which form the sides of the valley of the Nile exhibited, and in all their native lustre, various species of rocks; in the lower part limestone, farther up sandstone, and towards Syene, porphyry and granite. Egypt was in some measure a great mineralogical cabinet. The necessity of passing along the small valleys which run towards the Red Sea, led to the discovery of other minerals, which do not occur in so great masses. It was in one of them that the mine of emeralds was discovered, which supplied all those known in antiquity."

The discovery and working of the emerald mines, the only fact stated here, from which any thing can be inferred affecting the present subject, does not necessarily imply that the properties of minerals were tolerably well examined. Very barbarous nations, among whom not a trace of legitimate science has been discovered, have yet the propensity and the skill to dig out and ornament their persons with the natural gems; and it implies no more knowledge of mineralogy, much less of geology, in the ancient Egyptians, to dig mines for the emeralds, than it does in the inhabitants of Pegu to search for the rubies of their country, or in those of Siam for the sapphire. It may be allowed, yet within certain limits, that the properties of minerals were in some degree examined. It appears to have been known to them that their granites and syenites were more durable than their sandstones and limestones, as they have often carried the former from great distances to execute their more important architectural works, when they had the others nearer at hand;

but this implies no more than the knowledge possessed by every working mason, and from any thing communicated with certainty regarding them, we are not called on to ascribe to them more.

Egypt was no doubt, even in the most ancient times, as it is now, a great mineralogical cabinet, just as the Paris basin was. The minerals were placed there by the hand of the Author of Nature; but we have no more reason to believe that the ancient Egyptians could demonstrate and explain the order of the minerals in their country, than the Parisii, in the time of Julius Cæsar, could illustrate the Palæotheria and Anaplotheria of Montmartre.

But, in the lecture on the science of Egypt, the most unsatisfactory argument is that which relates to anatomy. In page 335, after stating that there were constant opportunities afforded of observing the external forms and habits of animals, as many were brought up in the temples of the gods, either as dedicated to them, or receiving divine honours themselves, it is added, " there were even occasions of observing their internal structure, as it was customary to embalm them after death;" and, " in Egypt the same horror towards dead bodies was not entertained as in India; not only were the bodies of sacred animals embalmed, but those of men also. Now, this practice could not fail to give those who were charged with it a knowledge of the form and position of the organs. It was undoubtedly in Egypt that anatomy originated; it was to that country that the Greeks resorted to study it; and thither Galen made a journey expressly for the purpose of seeing the representation in bronze of a human skeleton."

Now, with regard to this matter, when we reflect on the purpose for which the dead bodies were embalmed in Egypt, and what the motives must have been which led to the practice, we must immediately conclude, that, instead of affording facilities for acquiring a knowledge of anatomy, nothing could have presented a greater impediment to it. The purpose of embalming was to preserve the bodies as much as possible in the forms which belonged to them when alive, which was altogether incompatible with that dissection of the parts which unfolds their structure to the anatomist; the motive to the practice could be no other than the desire to honour the remains of their deceased kindred, which has been common to all the nations of the earth, and the effects of which, whether shewing themselves in embalming them, in depositing them in splendid catacombs or mausolia, or in simply interring them in grounds appropriated to that purpose, must be held as the natural and becoming expression of those family affections which form the basis of all human society. If, in the operation of embalming, the viscera were extracted, this was not for the purpose of investigating the structure of these organs, but for subjecting these more perishable parts to an additional preparation for preservation. In fact, there is no evidence whatever that anatomy was a science at all understood by the ancient Egyptians; and, with regard to comparative anatomy, the branch which has in later times illustrated geology, we have the statement of the Baron himself that Democritus of Abdera was the first who practised it. Even with regard to the science of medicine, which in all countries we find, in some shape or other, preceding anatomy, we have the testimony of Herodotus that it must have been at a low ebb in Egypt, when he tells us, that each of those who practised it applied himself exclusively to cure one disease, or the diseases of one organ. If Galen went to Egypt to see the representation of a skeleton in bronze, we must remember that this occurred long after the Alexandrian school of anatomy, under the patronage of a Macedonian race of kings had been enabled to throw some light, but still only a glimmering and very partial one, on that science, which it has been reserved for Cuvier himself to bring at last, in all its relations, into the full blaze of day.

When we reflect how absolutely the determination, in the present age, of the relative position of many of the strata of the earth, has depended on that beautiful comparative anatomy which, under the hands of Baron Cuvier, has become one of the best founded and most splendid monuments of the inductive philosophy, equally remarkable for the happy elucidation of both physical and final causes; we must at the same time acknowledge how impossible it was that Moses could derive his knowledge of the order of the epochs of creation from a people destitute of the incipient germs of the science, the full perfection of which alone could direct unaided reason to that knowledge.

In discussing the present question, we need scarcely refer, as the Baron has done, to some arts which the Egyptians possessed in a considerably advanced state, as the arts of making enamels and porcelains, of applying some excellent and durable colours in painting, and the art of representing the forms of animals, and man himself, in both their paintings and sculptures. The possession of these arts is not necessarily connected with any great progress in science, as is evident by the existence of some one or other of them, in much greater perfection, among several other nations, who yet can scarcely be said to cultivate what is properly termed science, as the Chinese and modern Hindoos.

In page 341, after a detail of the branches of natural science and art, in which the Egyptians are supposed to have made progress, but the evidence for which we find thus unsatisfactory, it is added: "It cannot be imagined that a nation which devoted itself with so much perseverance and success to the observation of nature, should have confined itself to the mere collecting of facts, without attempting to connect them by theories, and to ascend to principles. It must, therefore, be supposed that there was, at a certain epoch, in the colleges of the priests, the knowledge, not only of philosophical and religious doctrines, but also of particular scientific theories. These theories doubtless have been lost in consequence of the oppression to which the sacerdotal caste was subjected at the time of the conquest of Cambyses."

These conjectures, without proof, and in the avowed admission that all proof of them is wanting, are those which immediately precede the intimation that Moses had derived his knowledge of the cosmogony from the Egyptian priests. Here, then, is a singular series of mere suppositions. We must, *first*, suppose that all record and knowledge of the philosophical science of the priests was completely destroyed by the persecution of Cambyses, for no trace of it remains in any profane author; *secondly*, That the priests did certainly possess a truly philosophical science of geology, in perfection at least equal to that now attained by the joint labours and careful inductions of European geologists; *thirdly*, That Moses transcribed the results of this exact science into the 1st chapter of Genesis. And, *fourthly*, That, as we shall afterwards see, while he transcribed the accurate results, he carefully blotted out all traces of the successive steps by which they were attained.

But the evidence regarding the persecution of Cambyses, and its effects, does not warrant the conclusion that the science of the priests was destroyed by it. The most authentic evidence is to be found in Herodotus; and the account which he gives of it bears on its face so much of verisimilitude, that those of the after historians, where they differ from his, are extremely suspicious; especially when we consider that, by a reference to the preceding and accompanying and following circumstances, related by both him and them, it becomes obvious that his narration formed their chief authority. Herodotus, who visited Egypt only from sixty to eighty years after the time of Cambyses, and who takes care to inform us, that, with regard to the Egyptian history, after the time Psammatichus, he had the testimony not only of the Egyptians, but also of the Carian and Ionian colonies in that country, tells us that the persecution began only after the return of Cambyses from his disastrous expedition against the Macrobians. Nothing can be more natural than the account which he gives of its cause. Cambyses, at his return, found a universal rejoicing among the Egyptians, on account of the discovery of their god Apis by the priests. Suspicious that the rejoicing of the recently subdued nation arose out of the calamities of his own army, he inquired into the cause of it, and, being told what that was, ordered the newly found god, led by a priest, to be brought before him. The derision of the Persian fire-worshipper being excited by the appearance of a god in the shape of a calf, he wounded the animal with his own hand, but took so little interest in its further fate, that he never knew whether it died of the wound or not; but the rejoicing of the Egyptians, in the moment of his own calamity, excited not his derision, but his wrath. "He ordered," says the historian, "the priest to be whipped by his officers, and all those who were making public rejoicings, to be slain, wherever they were found." "Thus," adds he, "the priest was punished." Herodotus afterwards tells us, that Cambyses entered the temple of Vulcan, and mocked at the image of the god; and that he also entered the temple of the Cabirian gods, and having in like manner mocked at the images, ordered them to be burnt. This is the sum of the detail given by the historian nearest the time of that oppression to which the sacerdotal caste was subjected by Cambyses. Had it been of that terrible and exterminating character that would have involved the loss of their science, we should certainly have had from that historian a more full account of it; for he expresses great disapprobation of the Persian king for deriding religious institutions, and has given a very detailed and graphic account of the king's madness, and of his murders of many of his own Persians and relations.

The time that Cambyses remained in Egypt after the incident of the god Apis, was too short to admit of any extensive extermination of the priests and their science; for his whole reign was only seven years and five months, most of which must have been consumed in the negociations and preparations that preceded his Egyptian war, and the campaigns he made, including the delay occasioned by his embassy to the Macrobians.

Accordingly, Herodotus himself finds the priests still in full possession of their sacerdotal authority, and all branches of the Egyptian superstition flourishing in full vigour. He describes many of their temples as seen by himself still standing, and the scene of the same degrading rites to which they were originally destined; and the priests could yet shew him the spacious building which contained the images of all the high priests, from father to son, and could repeat their genealogies. Besides this, the priests were yet in possession of an accurate outline account of some part of the Egyptian history preceding the time of Cambyses, as is evident from the good agreement, in point of time at least, between the narrative which Herodotus received from them regarding Sennacherib and Necho and Apries, and the statements regarding these kings in the Jewish history *.

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* It is highly probable that we shall arrive ultimately at the conclusion, that the Sesostris of Herodotus is the same as the Shishak of the sacred writings. We are enabled, we see, by the histories of the Jews, to authenticate the outline of Herodotus' Egyptian history to about two hundred and sixty or seventy years before his time, and we find no such change or break in the character or arrangement of his narrative upwards to Sesostris, as, *prima facie*, to throw any more suspicion on the correctness of the general bearing of the early

It is altogether incredible, that, while their superstition and history thus survived the persecution of Cambyses, their science should have been utterly lost. There can be no question that it too survived, such as it was; and, regarding its true quality, the same inquisitive historian has furnished us with very considerable information. Thus, for instance, at his entering on the subject of Egypt, he puts us in possession of the true merits of their experimental philosophy, in the story of the two infants

part, than on that of the latter part. Now, in reckoning backwards from Sethon of Egypt and Hezekiah of Judah, who were both contemporary with Sennacherib, we find eleven kings of Egypt inclusively to Sesostris, and twelve kings of Judah to Rehoboam, who was contemporary with Shishak.

In estimating the merits of the position that Sesostris and Shishak (in the septuagint Sousakim) are the same, it is a matter of no small moment that the first syllables of the respective names are composed of letters of the same origin, especially as we find a similar circumstance taking place regarding the name of another Egyptian king, mentioned both by Herodotus and the Jewish historians; the Sethon of Herodotus being evidently no other than So (or, independently of the Masoretic points, Sua), in 2d Kings, xvii. 4, which the septuagint has changed to Segor.

There are several circumstances in which there are singular agreements between the details regarding Sesostris and Shishak. Herodotus tells us, that when Sesostris met with a brave resistance on the part of any nation, he erected honorary columns to commemorate their resolution, and when he was opposed by any people in a cowardly manner, he erected columns with marks of infamy upon them. He tells us, also, that he himself saw some of these columns of the latter sort in the Syrian Palestine, of which country we elsewhere learn from him the city Hadytis, or Jerusalem, was the capital; and on referring to the history of Shishak, we find that he was permitted to plunder Jerusalem without resistance.

The conquests of Sesostris and Shishak agree also in respect of their temporary character. Neither of them is described as attempting to keep permanent possession of the countries he subdued,—a very remarkable circumstance, almost unexampled in the history of conquerors.

The people named as forming the armies of Shishak agree well with what is said by Herodotus regarding the order of the conquests of Sesostris. We find among them the Cushim, by which name, it is well known, the early Hebrews denoted a people who dwelt in Arabia, near the Red Sea. Now, Herodotus states, that Sesostris made his first expedition with a fleet on the Red Sea, subduing the nations who dwelt on its shores. Hence, when he turned his arms to the north, he would have, according to the practice of conquerors, recruits in his armies from the conquered Cushim.

These remarks go far to prove that Herodotus had received from the Egyptian priest the true outline of the history of a period much earlier than Sennacherib.

who were ordered by the King Psammetichus to be brought up among the goats, without any one being permitted to speak to them, that it might be ascertained, by the words they should first utter, which nation was the most ancient. While they were thus inquisitive on this point, and took this hopeful method to settle it, they were utterly incurious regarding the most wonderful natural phenomenon of their country, the overflowing of the Nile. Regarding the causes of this, Herodotus tells us he could not get so much as a conjecture either from the priests or any other Egyptian, although he expressed to them a very earnest desire to learn something about them. He puts us, in another place, in possession of the accuracy of their conceptions regarding other natural phenomena, by telling us that the priests informed him, during the reign of their mortal kings, the sun had altered his course four times, had risen twice in the east and twice in the west; and, in full accordance with this singular specimen of their science, he tells us their astronomy was not their own, but that they had got it, and the use of the gnomon, and the division of the day, from the Chaldeans. It is unnecessary to go on to quote from the same source their opinion that fire is a fierce animal, eating up every thing, and then dying when it has no more to eat; or their sage conjectures, like those of the inhabitants of Laputa regarding the health of the sun, that the other nations must some time or other perish, as they depended on rain for rendering their soil productive, while that of their own country was rendered fertile by their river. In the face of this positive testimony of the utter worthlessness and ineptitude of their science and theories, furnished us by one so inquisitive, and who had the amplest means of acquiring the proper information on the spot, it would be quite unwarrantable to conjecture that they had any thing among them at all equivalent to our modern geology.

But independently of the testimony of Herodotus, after the time of Cambyses, we have some insight into the worthlessness of the Egyptian science, through Thales and Pythagoras, who both visited Egypt before the time of that invader. Before making a few observations on what we learn through them, we must enter a protest against what is stated in No. 17. of the

Edinburgh New Philosophical Journal, p. 43, that, "at the time when Thales went to study in Egypt, the priests of that country had already forgotten, in a great degree, the metaphysical doctrines which in former times were kept up in their colleges." Where is the proof of this to be found? Had the persecution of a former Cambyses obliterated all trace of these, and left to us a clear field to suppose they possessed them? Or is it to be found in the assumption that Moses had borrowed his metaphysics from the Egyptian colleges? Or is not this, strictly, reasoning in a circle, and begging a question at each extremity of the diameter? We shall afterwards see, that the conjecture that Moses borrowed his metaphysics from the Egyptians is altogether groundless; and we shall now return to Thales and Pythagoras.

We shall readily grant, for in doing so we pay no tribute of respect to the science of Egypt, that both these individuals learnt their peculiar dogmas in the colleges of the Egyptian priests. "Thales thought he had found in water a principle, that is to say, a thing pre-existent to every thing. According to him, water is the original matter from which the world is formed." His disciples were ungrateful scholars, for one found the first principle in infinity, another in air, and a third in fire. P. 43.—Pythagoras was even more refined: "He tried to discover the principle of things in the power of numbers." "He extended the language of arithmetic even to morals, and said justice was always divisible by two." "According to him, the universe was a harmonious whole, and on this account the number of the planets was equal to that of the notes of the gamut. In the centre of this harmony was the sun, the soul of the world, and the principle of motion. The souls of men and of animals participated in the nature of the celestial fire, and also those of the gods, who were themselves only animals of a superior order."—Pp. 44, 45. So, then, if Pythagoras got these notions in the Egyptian colleges, in which, according to the best accounts of him, he remained twenty-five years, we have a proof that their metaphysics was again recovered after the time of Thales; for here arises to our astonished view a complete system, not only of physics, but of metaphysics also.

comprehending the souls of men, and of gods, and of the world; but as it differs greatly from that of Moses, we shall not over rigorously insist that it was learnt in Egypt, but allow that it may have been the fruit of the Samian's own fertile invention.

The fact is, that in all this we find nothing that has relation or similitude to true science in any one department. It is acknowledged (p. 43), "that the experimental method was entirely unknown in these days." Equally wanting was every species of rational theory, which is in all cases the fruit only of experiment and observation. The dogmas of Thales and Pythagoras, whether indigenous of Greece or borrowed from Egypt, are only a small portion of that painful picture of human ignorance, weakness, and self-conceit, which the history of Grecian philosophy unfolds to us. The wildest hypotheses were assumed as the foundations of science, and the progress of true knowledge impeded by it for ages. Out of the abundant stores of his own genius and science, Baron Cuvier, as he has done in these lectures, may furnish a veil to cover the raggedness and squalor of its professors, and may make a subject amusing by his resources and eloquence, whose intrinsic merits can never render it interesting; but their capricious and fantastical hallucinations were long since better judged of by one of the most learned of our countrymen:—

"These are false, or little else but dreams,
Conjectures, fancies, built on nothing firm.
Alas! what can they teach, and not mislead,
Ignorant of themselves, of God much more,
And how the world began [*]."

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[*] There are only two exceptions to the insignificant and worthless character of the science of the early Greeks. One is their geometry, which will always remain a very beautiful and interesting monument of human genius. That geometry was invented in Egypt, is a conjecture of Herodotus, given as entirely his own. If the Greeks got it there, it must have been while it was yet quite in its infancy; for whatever credit we attach to the account that Pythagoras first demonstrated the theorem which forms the 47th proposition of Euclid, as referred to by the Baron Cuvier, we have much evidence that geometry was not much advanced till the period of the Alexandrian school. The geometry of solids must have been only quite in its infancy till the time of Archimedes, for we have the most complete historical evidence, that he first demonstrated the ratios of the cone, sphere,

There is another series of conjectures, founded on an obscure and unsupported hint of an Egyptian writer, and a few not well defined coincidences of arts and institutions of some ancient nations, and some acknowledged agreements in their languages, to which we shall now direct our attention.

In No. 16, p. 331, we are informed: "It is in India, according to all appearance, that we are to look for the origin of the sciences. It is in that country, in fact, that the men who escaped from the deluge must have established themselves. The loftiest mountains of the globe, the chains of Himalaya and Thibet, would afford them an asylum, and the bases of these mountains would present them with the first cultivable land. * * The priority of the Indians is farther shewn by a tradition to which no attention seems hitherto to have been paid. It is, in fact, in the extracts which have been preserved of the works of Manetho, that, in the reign of Amenophis, a king of the 16th dynasty, a colony came from India to settle in Ethiopia. Now Diodorus Siculus, and all those who have written on the religion of Egypt, derive that religion from Ethiopia, or Upper Nubia. Thebes itself was but an island, a colony of Meroe, which was the sacerdotal city of the Ethiopians. Thus, then, civilization came from India into Nubia, and from Nubia into Egypt." We are then informed, that among the Indians themselves we find no account of the progress of science, which is conjectured to be owing to the doctrinal point of the Brahmins, that history should not be written, although, in the case of Egypt, the conjecture had been, that science had been utterly lost, while we found history in some measure preserved. We then have a notice of the Indian monuments, which are acknowledged not to be very ancient; and of their most ancient books, the Vedas and Oupavedas, both of the date of 1500 years before Christ, the former containing an exposition of the religious philosophy of the Indians, the latter various scientific treatises, on music, medicine, war, architecture, and the mechanical arts; and it is added, "these two works are written in Sanscrit—a language which is

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and cylinder, inscribed in one another. The other exception is Aristotle's Natural History of Animals, in which naturalists recognise not a little that is valuable; but the valuable parts are obviously the work of his own mind, and not borrowed from Egypt or any other quarter.

not at present spoken—a language the most regular that is known, and which is especially remarkable for the circumstance, that it contains the roots of the various languages of Europe, of the Greek, Latin, German, Sclavonic; so that to find even the first instrument of science, namely language, it would seem we must go to the Indians in search of it." We have, then, an acknowledgment that the astronomy of the Indians is not very ancient, and that they had no knowledge of anatomy; so that, "in short," it is said, "all that the Indians could communicate to the Egyptians was their metaphysics, their mythology, and their constitution."

But if we admit the introduction of these branches into Egypt by the Indian colony, why must we exclude their other sciences, since they had music, medicine, war, and others. Is it lest we should be compelled to admit, along with these, their well-known absurd cosmogonies, and so deprive ourselves of the right to derive the cosmogony of Moses out of the utter darkness of Egypt? But let us briefly investigate the merits of the tradition preserved by Manetho, and how far it agrees with the true light of history.

The inquisitive Herodotus tells us, that he himself penetrated into Egypt as far as Elephantis, and there inquired particularly into the state and history of the countries higher up, and gives, as some of the results of his inquiries, that the Ethiopians of Meroe had no other gods but Jupiter and Bacchus, whom they worshipped with great pomp. Here, then, we find neither the mythology of India nor the fetishism of Egypt; and in consistency with this, he tells us in another place, that the crocodile, an object of adoration to the lower Egyptians, was so far from being held sacred, even no higher up than Elephantis, that the inhabitants were in the practice of eating him. Will these facts permit us to derive the religion of Egypt from Meroe? He gives us another piece of history, throwing much light on that of Meroe, and which he assigns to the reign of Psammetichus, from whose time, he says, the affairs of Egypt, through means of the Ionian and Carian military colonies, were nearly as well known to the Greeks as those of their own country. It is, that a large body of troops revolted from that king, and went to Meroe, where they were well received, and that through their means the Ethiopians were somewhat civilized, and learnt the manners of Egypt. Ought we to admit a vague tradition, committed to writing only at a much later period, in opposition to this circumstantial evidence of the progress of civilization from below upwards, instead of its coming in the contrary direction? Or can we be even entitled to set in opposition to it the opinion of Diodorus Siculus, who lived 400 years after the time of Herodotus?

The other proofs of a derivation from India will stand us in no better stead for supporting its reality, or even that of the derivation of the Egyptian religion from Meroe. Granting that much of the low part of Egypt was a gift of the Nile, p. 331, and that the land which is now the Delta was at an early period either a marsh or a lake, we must still remember, that the simplest reference to the order of nature, in such cases, leads to the certain conclusion, that on the margins of these there must always have existed a stripe of rich land, however narrow, watered yearly by a fresh-water river, and admitting a continuous population down to the neighbourhood of Palestine. We have thus a road always open for the introduction of the arts from that side, and are not reduced to the necessity of bringing them down through Meroe. Let us see the amount of probability that the architecture of Egypt, on which much stress is laid as being similar to that of India and Chaldea, was introduced through the lower or upper road. The Baron himself allows that the monuments of India, of gigantic proportions, may be judged to be posterior to the age of Alexander and the Ptolemies, p. 332. This at once cuts off all derivation of the Egyptian forms from India, and we are reduced to Meroe itself for their origin, if they came from the south. But the first time that the architecture of Egypt is presented to our notice, which is in the time of Moses, we find the buildings composed of bricks, evidently implying that it had its origin in the clay lands of the Delta or its neighbourhood.

It is allowed also by the Baron, p. 338, that most of the edifices of Egypt, which we know, must have been built from the year 1000 to the year 550 before Christ. But 500 years earlier than this, we find the inhabitants of Palestine possessing an architecture of stones, and cities described as walled up to heaven. (Numb. xiv. 40. Deut. i. 28). In the time of Samson we find columnar forms among the Philistines at Gaza. (Judges xvi. 25.) Above all, just at the period of the commencement of the Egyptian buildings, we find the Tyrians in possession of columnar forms, assimilated in their proportions to those of Egypt, and skilful enough to cast them in the costly material of brass. Hiram, the Tyrian, employed by Solomon in the construction of his temple, cast two noble columns of brass, having highly ornamented capitals, in the style common among the nations to whose architecture the Baron refers. (1 Kings vii. 15.)

We find Lucian also stating that the Phenician and Egyptian architectures resembled each other; and would it not then be wrong in point, not only of probability, but even of strict argument, after this clearly determined source for it, to search for the origin of the latter in Meroe. We shall find little difficulty in accounting for the appearance of the same forms in Chaldea, when we reflect that Nebuchadnezzar carried the inhabitants of Palestine to Babylon; little, too, for their being found at Persepolis and in India, when we further reflect that the Persians had such constant rule in Palestine, from the beginning of their monarchy; and that the Seleucidæ appear to have possessed for some time the Indian conquests of Alexander. After all, the fact with regard to the columnar forms may be, that those in different nations are not of one single origin, but were naturally derived in more nations than one, according to the opinion of eminent architects, from the wooden posts of the habitations which men first constructed.

The division into casts, which we find in ancient Egypt, and again in India, ceases to be a proof of common origin, and indicates even the contrary, when we find that these casts do not correspond with each other in the two countries. We find only four casts in India; and, according to Herodotus, there were seven in Egypt, three only of which are known in India.

After all, the languages of nations are the monuments which most certainly demonstrate either their diversity or community of origin. We readily allow, not that in the Sanscrit are to be found the roots of the European languages, but that a great number of roots are common to the former and the latter. Had a colony come from India to Egypt, we should find roots of all the European languages in the Coptic as well as in the Sanscrit; but none are found in the former, excepting such as are obviously derived from the Greek alone, and for the introduction of which into Egypt, the long possession of that country by the Macedonians sufficiently accounts. The proof, therefore, of any colonization of Egypt from India not only fails, but the results of our inquiry, in most of its branches, lead to the conclusion that no such colonization took place.

But failing the proof of the derivation of science from India, to the western part of the Old Continent through the road of Egypt, another path is pointed out by which it may have come to us from that country. We have already quoted the passage stating that the Sanscrit contains the roots of the European languages, and that it would seem we must go to India to find the first instrument of science language. We find, in a somewhat similar strain, at page 345, " The Pelasgi were originally from India, of which the Sanscrit roots that occur abundantly in their language, do not permit us to doubt." But why should this be predicated of the Pelasgi alone here, when we find the Greek, Latin, German, and Sclavonic, having Sanscrit roots? We are happily enabled, on sure grounds, to give a more satisfactory account of the origin of the relation between the Sanscrit and European tongues, than by conjecturing the arrival of a colony of Indians in Europe.

The " History of the European Languages," by the late Dr Alexander Murray, and the discovery of the Zend-avesta in the hands of the Parsees, have placed the whole question in a very simple and natural point of view. Dr Murray's book is, taken as a whole, unfortunately imperfect, not having been completed or arranged at the time of the author's death; and the learned editor having judiciously determined to present it as much as possible in the form in which it was left by so great a master of languages as the author, the more valuable parts of it consist of only disjointed notes, and it has not therefore attracted that notice which it deserves. Another circumstance which has proved unfavourable to a just public estimate of it is, that the author has attempted to establish a theory of the origin of the European languages, which indeed he has not been able to make good. But this can be easily separated from, and happily tends in no way to obscure, the flood of light which he has been enabled to throw over their affinities. Respecting this last, we need not go into any lengthened detail, but refer to the book itself. The roots and structures of the western and southern European languages, the Greek, Latin, Welch, Sclavonic, are traced up to the Teutonic, of which an old and authentic monument remains in a translation of the Scriptures, made in the fourth century, for the Mæso-Goths, a people inhabiting near the shores of the Black Sea, and not remote from the Thracian Bosphorus. Roots, similar to those found in the Teutonic, are pointed out in the few monuments of the Median and Persian languages, which have been preserved by the western historians, leaving no doubt that the Teutonic and these languages were very closely related to each other. The Zend-avesta, an ancient Median or Persian work, contains a profusion of the same roots.

A few facts that we can glean out of ancient history, confirm the conclusions deduced from these monuments of ancient languages. Thus in the Anabasis, we find Xenophon's interpreter, speaking to the Armenians in the language of the Persians; and in Herodotus' account of the army of Xerxes, we find him stating that the Armenians and Phrygians wore the same armour, and that the former of these were a colony of the latter; and also that the Phrygians had formerly inhabited in the country of the Macedonians. All this implies at least a common origin, and common language of the Persians, Armenians, Phrygians, and some early inhabitants of Macedonia.

We do not need, then, to call a colony out of India to plant the Sanscrit roots in Europe. We find them, at an early age, stretching along the eastern side of the Tigris, pointing on one hand towards Europe, through Asia Minor, and by the shores of the Black Sea, and on the other towards India through Parthia.

We thus discover, in very remote times, two great families of languages, very unlike both in their structures and roots, namely, the family having the European and Sanscrit roots, on the one hand, and that which includes the Semitic tongues, the Chaldee, Arabic, Hebrew, &c. on the other, marching with each other by a line which passes very nearly through the point assigned by Moses for the scene of the confusion of languages.

The Semitic languages, so peculiar in their structure and roots, having been thus limited, from early times, to the western side of the Tigris, forms an insurmountable objection to the conjecture that language had its origin in India.

In no evidence, therefore, extraneous to his own writings, is there any proof whatever that Moses derived his natural science from Egypt or India. When we refer to his own writings, we discover there highly satisfactory internal evidences that he did not, and to some of these we shall now direct our attention.

The first chapter of Genesis is written in a pure Hebrew. This was the language spoken, and afterwards extensively written, by the people whom Moses conducted to Palestine from the land of Goshen. That it differed greatly from the language of the Egyptians, we have full proof in the Coptic remains of the latter, in the Egyptian proper names preserved in the Hebrew writings, and also in the circumstance that Joseph, when pretending to be an Egyptian, conversed with his brethren by means of an interpreter. Yet, in the chapter in question, we find no foreign terms, no appearance of its being translated from any other tongue; but, on the contrary, it bears every internal mark of being purely original, for the style is condensed and idiomatical in the very highest degree. Had Moses derived his science from Egypt, either by oral communication, or the study of Egyptian writings, it is inconceivable that some of his terms, or the style of his composition, should not, in some point or other, betray the plagiary or copyist.

But the conjecture that Moses borrowed his cosmogony from the Egyptians, must rest, moreover, on a supposition that the order which he assigns for the different epochs of creation had been determined by a course of observation and induction, and the correct application of many other highly perfected sciences to the illustration of the subject, equal at least in their accuracy and philosophical precision to those by which our present geological knowledge has been obtained. Nothing less than this can account for Moses' teaching us precisely what the modern geology teaches, if we allow his knowledge to be merely human.

How comes it pass, then, that while he has given us the perfect and satisfactory results, he has been enabled so totally to exclude from his record every trace of the steps by which they were obtained? The supposition of such perfection of geological knowledge in ancient Egypt, implies a long series of observation by many individuals having the same object in view. It implies of necessity, also, the invention and use of many defined terms of science, without which there could have been no mutual understanding among the different observers, and of course no progress in their pursuit. These terms have all totally disappeared in the hands of Moses. He has translated, with precision, the whole science of geology into the language of shepherds and husbandmen, leaving no trace whatever of any one of its peculiar terms any more than of the curious steps in its progress.

But there is a phenomenon in his record still more unaccountable upon any supposition that his science is merely human. His geology, acknowledged by the highest authority in this age of scientific improvement to be thus accurate, dwindles down in his hands to be a merely incidental appendage to an enunciation of the most rational and sublime theology. This latter he did not learn in Egypt, for it was in possession of his ancestors while they were yet inhabitants of Canaan; and we find Fetishism established in Egypt in his age, and even as early as the time of Joseph. Joseph's steward addresses his brethren as if their God were different from the gods of Egypt (Gen. xliii. 23.), and we find him afterwards stating (Gen. xlvi. 34.), that every shepherd is an abomination to the Egyptians. Herodotus has given us a piece of information, which forms a perfect commentary on this last passage, and puts us in possession of all its import. He tells us that cows, whether young or old, were, by the Egyptians, all held sacred to Isis, and were forbidden to be sacrificed; and that on this account, they more venerated that animal than any other; and he adds almost a paraphrase of the words of Joseph, " therefore, no man or woman of them will kiss a Grecian, or use his knife, or pot, or spit, or eat the flesh of animals cut with his knife." The Greeks were thus an abomination to the Egyptians, because they sacrificed the animal sacred to Isis. Now, the Hebrews were in the practice of sacrificing the same animal, for we find a heifer among the sacrifices of Abraham (Gen. xv. 9.) The proofs of the existence of Fetishism in Egypt in the time of Moses, and that the Egyptians knew not the God of the Hebrews, are complete. In Exodus viii. 26., we find Moses saying to Pharaoh, "shall we sacrifice the abomination of the Egyptians before their eyes, and will they not stone us?" In Exodus xii. 12. we find the beasts called the gods of Egypt; and in Exodus xxxii., we find Aaron, in consistency with the idolatry which he had witnessed in Egypt, making a golden calf, and saying to the Hebrews, this is thy God. Also, when Moses first presents himself before Pharaoh in Exodus v., we find the latter denying all knowledge of the God of the Hebrews.

Shall we, then, conjecture that Moses borrowed theology from the Hebrews on the one hand, and geological science from the Egyptians on the other, to compound out of them that brief but unique and perfect system of both, which is presented to us in the 1st chapter of Genesis; or, is it possible that we could adopt any conjecture more absurd, and this, too, in utter destitution of all proof that the Egyptians possessed any knowledge of geology in the sense in which we use the term?

The result of our inquiry is, that the geology of Moses has come down to us out of a period of remote antiquity before the light of human science arose; for, to suppose that it was borrowed from, or possessed by, any other people than the remarkable race to which Moses himself belonged, involves us on all hands in the most inextricable difficulties and palpable absurdities*. Of that race, it has been long since justly remarked,

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* We believe that the opinion of Calmet may be maintained by very extensive and highly satisfactory internal evidence, that Moses, in the book of Genesis, has transmitted to us the successive writings of the earlier Patriarchs, just as the Prophets, who succeeded him, have transmitted to us that book and his own writings. We believe, likewise, with Bishop Gleig, that the opinion generally entertained of the late invention of alphabetical writing, is no other than a vulgar error, and that such writing must have been practised before the flood of Noah.

Sir William Jones, when he hazarded the conjecture, or rather opinion, that the language of Noah is probably entirely lost, must have quite overlooked the internal evidences, that the original language of Geneses can be no that while in religion they were men, in human learning and science they were children; and if we find in their records any perfect system of an extensive and difficult science, we know they have not obtained it by the regular processes of observation and induction, which, in the hands of European philosophers, have led to a high degree of perfection in many sciences.

Let us now, then, inquire into the true value and necessary result of Baron Cuvier's statement, "that the cosmogony of Moses assigns to the epochs of creation precisely the same order as that which has been deduced from geological considerations."

Before we proceed to that detail and comparison of particulars which are necessary in the due prosecution of the inquiry, we purpose to shew that a right understanding of the terms employed by Moses will lead us to several more agreements between his statements and the results of the modern geology, than are indicated by our common English translation. This will lead us into a critical examination of several of these terms. We do not mean to hinge much of these criticisms on grammatical niceties, but to rest them chiefly on an examination of other passages of the Hebrew Scriptures, where the terms are also employed, and where the context throws such light on them as puts an end to all doubt about their true import. This is a process of criticism which is universally allowed to be quite satisfactory, where we have resources for employing it, as happens to be the case in the present instance.

To make our criticisms intelligible, without the labour of turning to the passages quoted, we shall quote the common English translation to such an extent as may be necessary.

The term, the meaning of which we shall first investigate, is "*day*" (in the Hebrew, yom). The interpretation of this, in the sense "*epoch*" or "*period*," has been a subject of animadversion, of an unnecessary severity in some cases. A careful examination of the first chapter of Genesis itself leads unavoidably to the conclusion, that our natural day of one revolution of the sun cannot be here meant by it, for we find that no fewer than three of the six days had passed before the measure of our

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other than the language of both Noah and Adam. But these questions are too important and extensive to be more than thus briefly alluded to in a note.

present day was established. It was only on the fourth day, or epoch of the creation, " that God made two great lights to divide the day from the night, and to be for signs, and for seasons, and for days, and for years." The very first time that the term occurs in the Hebrew text, after the history of the six days' work, and of the rest of the seventh, as if to furnish us with definite information regarding its true import, we find it employed in a similar manner to that in which we must understand it here; for, in Gen. ii. 4, we have, " These are the generations of the heavens and the earth, *in the day* (beyom) that the Lord God made the earth and heavens." The use of the term in this indefinite sense is so common in the Hebrew writings, that it would be a great labour to quote all the passages in which it is found; and we shall satisfy ourselves by at present referring to Job xviii. 20, where it is put for the whole period of a man's life, " They that come after him shall be astonied at *his day*" (yomu); and Isaiah xxx. 8, where it is put for all future time, " Now go write it in a book, that it may be for the latter *day* (leyom), for ever and ever." It is quite obvious, from these examples, that the Hebrews used the term (yom) to express long periods of time. The very conditions of the history in this chapter prove that it must be here so understood.

They who object to this interpretation of the term here, immediately quote against it the reason added to the fourth commandment, " For in six days the Lord made heaven and earth, the sea, and all that in them is, and rested the seventh day, wherefore the Lord blessed the Sabbath-day and sanctified it." This is, however, no more than a brief reference, and the terms of it must therefore be strictly interpreted in accordance with those of the detail to which the reference is made.

It has been said that such an interpretation goes to nullify the reason assigned for the sanctification of every seventh revolution of the sun; but this does not follow. In point of fact, the rest from the work of creation (we use this form of speech from the example before us) did not endure only for one revolution of the sun, but has continued since the creation of man; and we have no grounds on which to establish even a conjecture of the time of its coming to a close; so that if we were urged to adopt a period of twenty-four hours as the meaning of yom, that the six days of creation might literally correspond with our six working days, we should then find the apparent disagreement, which, by this process, we would endeavour to avoid, transferred to our weekly period of rest, and the rest from the work of creation.

It will surely be readily allowed, that the sanctification of the Sabbath has respect to man and his duties; and since his Creator has been made known to him, and the order of the six successive epochs in which the earth was rendered fit for his habitation; if we are to allow, what surely no reflecting mind will ever deny, that it is his duty to reflect with gratitude on the blessing he has received, and to maintain in his heart a sense of his dependence upon, and responsibility to him, who made the heavens and the earth, and all that they contain, no method could have been devised better calculated for preserving these feelings in constant activity than appointing some definite portion of time, returning at short intervals, to be devoted to the contemplations that awaken them, nor any interval more appropriate than that which so directly recalls the order of the events of the creation.

Since we have here introduced the subject of the measure of our present day, we would offer an observation regarding the work of the fourth day, which includes the sun, moon, and stars. Respecting the period of their creation, geology, from its nature, gives us no precisely definite indications. The history regarding them is from the 14th to the 18th verses, and we would observe of it, that the terms employed are such as do not absolutely imply that these bodies were at this epoch first created, but admit of the interpretation that their motions were then first made the measures of our present days and seasons. We had found it already stated in the 1st verse, that the heavens and the earth were created *in the beginning*, antecedently to the work of the six days, by which they were reduced to their present order, and the earth was peopled with organized beings. It would seem an unwarrantable interpretation to exclude the sun, moon, and stars from among the objects expressed by the general terms, the heavens and the earth. It is the most obvious interpretation, that they were then created, and were lighted up on the first day, but that it was only during the fourth epoch that they were made, the greater light to rule over the present day, and the lesser light to rule over the present night, and to be for signs, and for seasons, and for days, and for years, according to the measures of time which we now find established by them. This part of the history, then, when interpreted in consistency with the 1st verse, and without any violence to the terms, implies, in the common language of men, which, in all nations, refers the diurnal and annual revolutions of the heavenly bodies to the motions of these bodies themselves, that the earth was, during this epoch, finally brought into its present orbit.

The work of the third epoch was the appearance of the dry land, and the creation of the vegetable kingdom. The history of the latter, in our common translation, is, v. 11, "God said, Let the earth bring forth *grass* (in the margin *tender grass*), the herb yielding seed, and the fruit-tree yielding fruit after his kind, whose seed is in itself, upon the earth: and it was so." V. 12, "And the earth brought forth grass, and herb yielding seed after his kind, and the tree yielding fruit, whose seed was in itself, after his kind." The terms grass (in Hebrew, deshe), herb (Hebrew, oeseb), and tree (Hebrew, etz), are here all put disjunctively in the Hebrew; there being only one conjunction in the twelfth verse between herb and tree, which does not affect the disjunctive character of the three terms, as it is a common practice in the Hebrew writings to couple, in this manner, the two last of a series of disjunctive terms, as, for example, the names of the four kings in Genesis xiv. 1. In the two last of these terms, herb and tree, we find a recognition of a remarkable natural distinction among the vegetable tribes, and this very circumstance would lead us to infer, that the first term, which has obviously presented a difficulty to our translators, since they have given two interpretations of it, is intended to express some class or tribe of the vegetable kingdom, naturally distinguished from herbs and trees, as they are from one another. The term in question (deshe) is a noun from a verb, which, from Joel ii. 22, we learn the meaning is *to spring*, *to shoot*, *to vegetate*, "Be not afraid, ye beasts of the field, for the pastures of the wilderness *do spring* (dasheu)." In the 11th verse under consideration, we find both the verb and the noun, for the words translated "*Let the earth bring forth*" are (tadeshe haaretz), which, in accordance with the obvious sense in Joel, would be better rendered "*Let the earth shoot out.*" From this meaning of the verb, then, the noun would signify the *springing* or *shooting plant*, and as used here in contradistinction to both herbs and trees bearing seeds, it is surely not recommending any forced interpretation to suggest that it is meant to express that class of vegetables, which all botanists recognise as being naturally distinguished by the obscurity of their means of reproduction.

It tends to support this interpretation, that the Hebrew has a different term for grass, the common food of cattle (chatzir), which the lexicographers have shewn is derived from its tubular structure. Thus, in Job xl. 15, we have "he eateth *grass* (chatzir) as an ox;" and, Psalm civ. 14, "He causeth *grass* (chatzir) to grow for the cattle."

In several passages besides this of Genesis, we find *deshe* contradistinguished from both *oeseb* and *chatzir*, as in Deuteronomy xxx. 2. "As the small rain upon the *tender herb* (deshe), and as the showers upon the *grass* (oeseb);" and Psalm xxxvii. 2, "They shall soon be cut down like the *grass* (chatzir), and wither like the *green herbe* (deshe);" and 2d Kings xix. 26, "They were as the *herb* (oeseb) of the field, as the *green herb* (deshe), as the *grass* (chatzir) on the house tops." These quotations shew the want of uniformity with which the English translators have rendered these terms, and go to support the sense we would assign to *deshe*.

But we must not conceal that there are three passages in which this word occurs, that might seem to imply, until closely examined, that we should not be warranted to restrict the sense of it in the manner proposed. One is in the 23d Psalm, "The Lord is my shepherd, I shall not want. He maketh me to lie down in the pastures of *tender grass* * (deshe)." On this we have to observe, that the word rendered here *in the pastures*, has been rendered by the Vulgate, in various places where it occurs, and by the Septuagint in some instances, *desirable* or *beautiful places*, and their accuracy in doing this seems confirmed by the circumstance, that the Hebrew has another term for pasture; and if this interpretation of that word be admitted, then *deshe* might signify here plants rather fitted for lying down on, as the mosses and ferns, than for pasture, which would make out a consistent image expressed in this clause or sentence, in opposition to the one derived from the abundance of pasture, which is evidently already sufficiently completed in the terms, "The Lord is my shepherd, I shall not want." This passage, then, when rightly understood, rather serves to confirm the meaning which we have suggested for *deshe*. Another passage is Job vi. 5, "Doth the wild ass bray when he hath *grass* (deshe), or loweth the ox over his fodder?" but no stress can be laid upon this, when we consider that both the ass and the horse eat, of choice, various species of ferns and equiseta, a fact which it is not unreasonable to suppose might be known to the author of a book which contains so much accurate and interesting natural history as this of Job. The plants, whatever they might be, which formed a supply for the wild ass, are at least obviously set in contradistinction to those which formed the fodder of the ox. The third passage is Jeremiah l. 11, "because ye are grown fat as the heifer at *grass* (deshe)." But there is in a great number of manuscripts a various reading for *deshe* here, by which the meaning becomes, "ye are grown fat, like a heifer thrashing, or treading, out the corn;" and several circumstances shew the latter reading to be the more probably correct one.

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* The marginal translation, which is the literal one.

It remains, then, very highly probable, upon the whole, that *deshe*, in the 11th and 12th verses, is intended to express the cryptogamous vegetation.

In our observations on the terms employed in the history of the creation of the animals, we shall arrive at some important conclusions that are more absolutely certain.

The first thing that we would observe in regard to this, is, that there are two distinct words, of very different origin, which the English translators have rendered, promiscuously, *creeping creatures* or *things*, and also *moving creatures*, following, no doubt, the authority of the Septuagint, which has given ἑρπετα for both; thus occasioning a great confusion instead of a clear and perspicuous order of creations exhibited in the Hebrew text. The first of these words is *sheretz*, as in verse 20th, in the history of the fifth day's work, " God said, Let the waters bring forth abundantly the *moving creature* (sheretz)," in the margin the *creeping creature.* This word is from a verb, which signifies to *bring forth* or *to increase*, or to *multiply abundantly*, being the very verb which is rendered *bring forth abundantly* in the 20th verse, " Let the waters bring forth abundantly," (is heretzu hamaim). We find the verb obviously having this meaning in other passages, of which we shall quote examples : Gen. viii. 17, " *That they may breed abundantly* (vesharetzu) in the earth, and be fruitful and multiply in the earth ;" Exod. i. 7, " And the children of Israel were fruitful, *and increased abundantly* (vaisheretzu), and multiplied, and waxed exceeding mighty, and the land was filled with them ;" Exod. viii. 3, " And the river *shall bring forth* frogs *abundantly* (vesharatz), * * * and the frogs shall come up both on thee and on thy people, and upon all thy servants."

From all this it appears that the proper translation of the noun sheretz is not *the creeping*, but *the rapidly multiplying creature.* The creatures expressed by this noun were part of those which were created during the fifth epoch.

The other word translated *creeping thing* is (remes), and the creatures expressed by the noun were created during the sixth epoch. We shall afterwards shew that it has a very different meaning from sheretz.

In the history of the fifth day's work the translators have rendered the Hebrew word (oph), by fowl. This limits its meaning so as to include only the birds. But the term includes also the winged insects, as is evident from Leviticus xi. 20, " All *fowls* (haoph) that creep, going upon four."—The proper translation of the term is not *fowl* but *flying thing*, including the tribes of all kinds that can raise themselves up into the air ; as is indeed rendered obviously by the expression in the 21st verse of the 1st chapter of Genesis itself (cal oph canaph), " every flying thing that hath wings."

In the 21st verse it is said, " God created (hathananim hagedolim)," which Hebrew words, our translators, following the Septuagint, which has given for them τα κητη τα μεγαλα, have rendered great whales. We have abundant resources to shew that this translation is erroneous. In fact, neither the Greek nor the English translators have been consistent with themselves in translating the Hebrew word (than) or (thanin), for it occurs in both these forms. We find them in other places translating it severally by the terms δρακων, and dragon. It would be tedious to quote the passages where they have thus varied from themselves. We shall refer to Ezekiel xxix. 3. for the latter sense, "I am against thee, Pharaoh, King of Egypt, *the great dragon* (hathanim hagadol) that lieth in the midst of his rivers," where the Septuagint has τον δρακοντα τον μεγαν. The figure in this passage is evidently borrowed from the crocodile of the Nile, and this circumstance of itself would shew that dragon, in place of whale, would be a better translation in Genesis. But (thanin) has a still more comprehensive meaning. We find two words formed from it, one of which (Leviathan) is the specific name of the crocodile, as is obvious from descriptions of Job chap. xli. and of Isaiah chap. xxvii. 1, in which last passage (thanin) is also used,—and the other (Pethan) is the specific name of some serpent, as is obvious from the reference to its poison, in Job xx. 14, and Deuteronomy xxxii. 33. In this last passage we also find poison ascribed to the thanin; "Their wine is the poison of dragons (thaninim), and the cruel venom of asps (pethanim);" so that here it is evidently meant to express a serpent, as in Ezekiel and Isaiah, as we have seen above, it signifies one of the lacertine species.

These references, which we could have greatly extended, were it necessary, are sufficient to prove that (than) or (thanin) was a sort of generic, or rather classical, name, to designate the serpent and lizard tribes; and that instead of *great whales* in the 21st verse, the translators should have given the words *great reptiles* *.

The result of our criticism is, that the work of the fifth epoch, as described in Genesis, was the creation of the inhabitants of the waters; of the birds, winged insects, and reptiles; in fact, of the oviparous races named in detail, with some omissions, which are to be accounted for by the uniformly condensed and brief form of the whole narration.

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* There is only one passage in which (than) means, with certainty, any thing else than a serpent or reptile, which is Lamentations iv. 3, where probably a seal is meant; but the passage is highly poetical, and no authority can be given to it to supersede the uniform meaning of the term in all the earlier writers, which we have established in the text.

We proceed to the work of the sixth epoch, which concluded with the creation of man.

In the English translation we find *creeping things* again included among the beings which were created during this period, and these English terms, in their most commonly received acceptation, imply some of the insect or reptile tribes. We have seen that the Septuagint countenances the interpretation *creeping things;* but the Hebrew term (remes) does not. This is derived from a verb which signifies *to move*, and which is so far from being limited in its application to the insects or the reptiles, that, in Psalm civ. 20, 21, we find it applied to the beasts of the forest and the young lions: "Thou makest darkness and it is night, wherein all the beasts of the forest *do creep* (tiremos). The young lions roar after their prey." In the 24th and 25th verses, (remes) is grouped with cattle (behemach), and *beast of the earth* (haith haaretz). Proofs are abundant, and too tedious to be all referred to, that by (behemah) the Hebrews generally expressed the larger herbivorous animals, and by (haith haaretz) the larger beasts of prey. (For the former see Genesis xxxiv. 23, and for the latter Leviticus xxvi. 22). Thus we find races of mammalia expressed by these terms, and to comprehend the whole class we must understand (remes) as referring to its other tribes. It is at least no race of insects that can be meant by the term, for, in point of fact, where any of these are obviously meant in other Hebrew passages, either the name (sheretz) is given to them as in Leviticus xi. 42, "Whatsoever doth multiply feet among all *creeping things*," (hasheretz), or the name (oph), as we have already seen.

It is true that remes is applied to the oviparous tribes, but not as a noun or name, but as a verb to express their motion, just as in some passages above quoted, we have seen sheretz applied as a verb, but not a name to mammalia.

Previously to setting down the following table of coincidences between the 1st chapter of Genesis and the results of geological observation, it is necessary to make a remark on one passage in Humboldt's table of geological formations, which possesses a classical celebrity over Europe. In that table, following an earlier authority, he has placed the formations of transition, in the limestones of which are found several species of shells, intermediately between the primitive formations and those containing bituminous coal; and his table would thus indicate that an animal creation had preceded any vegetable one. We shall not need to discuss the question, whether the formations named Transition are considered in a right point of view, when they are placed between the primitive and pit-coal strata, since it is sufficient for our present purpose to remark, that several observations, among which we may particularly refer to those of Thomas Weaver, Esq. F. R. S. on the geological relations of the south of Ireland, have proved that the anthracite or glance-coal of the transition formations, with some of its accompanying strata, are full of impressions of various plants; so that in the transition strata a vegetable creation is discovered as well as an animal.

In the following table we have taken the geological facts from various authorities. The passages quoted are selected chiefly on account of their brevity. In the quotation from and reference to Genesis, the events on which geology can throw no certain light are in italics.

TABLE of Coincidences between the Order of Events as described in Genesis, and that unfolded by Geological Investigation.

In Genesis.	No.	Discovered by Geology.
Gen. I. 1, 2, In the beginning God created the heavens and the earth. And the earth was without form and void; and darkness was upon the face of the deep: and the Spirit of God moved upon the face of the waters.	1	It is impossible to deny, that the waters of the sea have formerly, and for a long time, covered those masses of matter which now constitute our highest mountains;
3, 4, 5. *Creation of light.* 6, 7, 8. *Creation of the expansion or atmosphere.* 9, 10. Appearance of the dry land.	2	and, further, that these waters, during a long time, did not support any living bodies. (Cuvier's Theory of the Earth, sect. 7.)

Table—continued.

In Genesis.	No.	Discovered by Geology.
11, 12, 13. Creation of shooting plants, and of seed-bearing herbs and trees.	3	1. Cryptogamous plants in the coal strata (Many observers.) 2. Species of the most perfectly developed class, the Dicotyledonous, already appear in the period of the secondary formations, and the first traces of them can be shewn in the oldest strata of the secondary formation; while they uninterruptedly increase in the successive formations. (Professor Jameson's remarks on the Ancient Flora of the Earth.)
14 to 19. *Sun, moon and stars made to be for signs, and for seasons, and for days, and for years.*		
20. Creation of the inhabitants of the waters.	4	Shells in alpine and Jura limestone. (Humboldt's tables.) Fish in Jura limestone. (Do.) Teeth and scales of fish in Tilgate sandstone. (Mr Mantel.)
Creation of flying things.	5	Bones of birds in Tilgate sandstone. (Mr Mantel, Geological Transactions, 1826.) Elytra of winged insects in calcareous slate at Stonesfield. (Mr Mantel.)
21. The creation of great reptiles.	6	It will be impossible not to acknowledge as a certain truth, the number, the largeness, and the variety of the reptiles, which inhabited the seas or the land at the epoch in which the strata of Jura were deposited. (Cuvier's Ossem. Foss.) There was a period when the earth was peopled by oviparous quadrupeds of the most appalling magnitude. Reptiles were the lords of the creation. (Mr Mantel.)
24, 25. Creation of the mammalia.	7	Bones of mammiferous land quadrupeds, found only when we come up to the formations above the coarse limestone, which is above the chalk *. (Cuvier's Theor. sect. 20.)
26, 27. Creation of man.	8	No human remains among extraneous fossils. (Cuvier's Theor. sect. 32.) But found covered with mud in caves of Bize. (Journal.)
Genesis VII. The flood of Noah 4200 years ago.	9	The crust of the globe has been subjected to a great and sudden revolution, which cannot be dated much farther back than five or six thousand years ago. (Cuvier's Theory, sects. 32, 33, 34, 35, and Buckland's Reliq. Diluv.)

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* *Note.*—One solitary exception is since discovered, in the calcareous slate of Stonesfield, in the bones of a didelphis, a tribe whose position may be held intermediate between the oviparous and mammiferous races.

In the above Table, we have not taken advantage of the distinction which, we conceive, we have gone far to prove, is expressed in the Hebrew text between the cryptogamous and the other classes of plants, but have set down the whole vegetable kingdom as forming only one element in the table. We shall also allow that the 4th, 5th, and 6th Nos. may be liable to be interchanged among themselves, in respect of place, and shall hinge no argument upon them, farther than what arises from the circumstance that they are all placed in one group. Yet, after these abatements from the number of particulars, the coincidences here shewn between the order of the epochs of creation assigned in Genesis, and that discovered by geology, are calculated to excite the deepest attention. Human science, in the probability of chances, as illustrated by La Place, has put us in possession of an instrument for estimating their value; and we feel amply entitled to take advantage of it for that purpose, for no case could well be pointed out, where it would be more correctly applicable than in this, where the coincidences assume a definitely successive numerical form. We are entitled to adopt even the very language of Laplace, and to say, "By subjecting the probability of these coincidences to computation, it is found that there is more than sixty thousand to one against the hypothesis that they are the effect of chance *."

It is thus, then, that the discoveries of geology, when more matured, instead of throwing suspicion on the truths of revelation, as the first steps in them led some persons to maintain, have furnished the most overpowering evidence in behalf of one branch of these truths. The result of these discoveries has been in this respect similar to those of the Chinese and Egyptian histories, and the Indian astronomy, but much more striking. Eminent men had pledged their fame in setting up these histories, and that astronomy, in opposition to the chronology of Genesis; but further and more careful inquiry into their true characters, discovered that, when rightly understood, they only tend to confirm it.

We are not afraid that we shall have here quoted against us the words of Bacon, "Tanto magis hæc vanitas inhibenda venit, et

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* Syst. du Monde, book v. chap. 6.

coercenda, quia ex divinorum et humanorum, male sana admixtione, non solum educitur, philosophia phantastica, sed etiam religio hæretica." We have only endeavoured to illustrate and point out the consequences of the statement of Baron Cuvier, " that the order which the cosmogony of Moses assigns to the different epochs of creation, is precisely the same as that which has been deduced from geological considerations." We have been guilty of no improper mixing up of divine and human things. We have examined the meaning of the terms in the first chapter of Genesis, in consistency with the acknowledged rules of criticism, and only by the light contained within itself, or that thrown upon it by the other books, in the same language with which it is associated. The human science we have not extracted from any part of the Holy Scriptures; we have taken it simply as we find it in the works of eminent geologists. As the latter is not a philosophia phantastica, but a deeply interesting science, constructed by that method of careful observation and cautious induction, which Bacon was himself the first to recommend; so neither can the sense of the Scriptures present to us a religio hæretica. If our science, thus constructed, and our religion speak so obviously the same language, as we have seen they do on one important point, what else, in the strictest application of Bacon's philosophy, can we deduce from the circumstance, but that both are certainly true?

It does not come under our present subject to discuss the historical and moral evidences of the divine revelation of the Scriptures; but both are so full, even to overflowing, and impose upon us so many insuperable difficulties, in the way of our being able to account for the quality and consistency of these remarkable books, excepting on the ground which has been all along assumed by themselves, that they are of more than human origin, that in estimating the accuracy of any part of the matters contained in them, the fastidiousness of human science appears to be carried to an unreasonable extent, not to take these evidences into calculation. In this country, where for a long period we have had the Scriptures in our hands as a popular book, they among us who have been the most eminent for human learning and science, and whose fame has been in every view the most unsullied, have been so convinced by the force of these evidences, that they have in general been the most strenuous defenders of revelation.

Will not human science, then, condescend to borrow some light, to direct the steps of its own inquiries, from a record, the accuracy of which it has itself proved, and which is supported by other proofs of the highest order? or, what should we say to the illustrator of the relics of Pompeii and Herculaneum, who should reject the light thrown on them by the Letters of Pliny, authenticated as these are by the existing remains of the buried cities, as well as the historical evidence which is proper to themselves?

Among the questions which geology is at present attempting to solve, is that of a different temperature of some regions of the earth at a remote age. The discoveries of Pallas and Adams, of a rhinoceros and elephant in Siberia, having coverings of hair fit to protect them from the cold of the northern regions, would seem to decide the question, so far at least as to shew, that there has been no change of temperature since the creation of animals. But the question does not seem yet so satisfactorily answered, so far back as to the age of the creation of vegetables. Does not the statement in Genesis, that the establishment of our present days and seasons was intermediate between the creation of vegetables and that of animals, give us a clew to direct our path in the inquiry?

On the Fundamental Types of Organization. By G. R. TREVIRANUS, M. D. &c.

THE doctrine of organization is founded on comparative anatomy, or the systematic distribution of living bodies, and on organic chemistry. It is not to be expected that we are to give here any thing but a mere outline of these sciences; a few landmarks, which I think have some pretensions to novelty, and are more correct then those which have been hitherto most generally admitted.

We can arrive at no mutual understanding in biology until accurate definitions are given of the classes, families, genera, and species of living beings. Ever since natural history has been something more than a mere crude digest of unconnected facts and observations, philosophers have constantly endeavoured after the discovery of an arrangement in which the objects of their inquiries should be linked after their natural affinities; while, at the same time, the characters of its subdivisions should possess the utmost simplicity, by being derived from a single system of organs. It may, in a manner, be compared with the philosopher's stone, whether he merely resort to such an arrangement for the purpose of discovering the name of an unknown animal or plant, or whether, with higher views, his researches enter deep into the philosophy of nature. With the first object only in view, external characters alone may suffice, and the more easy these are of detection, the better are they fitted for their purpose. The bond of natural affinities is quite subordinate to this primary object. But if his object be of the other description, the characters may lie deep, and be of the utmost difficulty to discover. His arrangement will be the more perfect the more completely it expresses the sum of all the external and internal structural differences, and the more uniform the parts are from which the characters are taken.

Of late years systems have been constructed upon the last of these principles. But their bases have been always such as I cannot admit. One of these is the principle, That all higher formations include those lower in the scale; the most perfect organs in the former having already existed in the latter, but in their undeveloped state; that there is one universal type, only modified in the degree of its development. There is much, both of truth and of error, in all this. It is true that every organized being advances from the simpler to the more compound in its progressive growth, and that the early stages of the life of an animal high in the scale present many points of similarity with the perfect state of another lower in the series. But these resemblances hold merely in external relations, and the numerous points of difference can by no means be overlooked.

The fœtus of the mammalia and of birds is an aquatic animal, which respires not by means of lungs, and as such has a simple circulation like a fish. But in all other respects its circulating system is constructed upon a totally different principle from a fish. The apparent analogy vanishes upon a closer inspection.

The brain of the fœtus of the mammalia apparently resembles that of the other three vertebrated classes, as the tubercula quadrigemina of the former, like the posterior hemispheres in the latter, exceed in volume all the other parts. But these posterior hemispheres contain parts which have nothing in common with the tubercula quadrigemina. In the early states of mammalia and fishes there are fissures in the sides of the neck which are somewhat similar to the external branchial orifices of the larva of the frog, tod, and salamander. But the resemblance is merely external. The fissures conduct to no true gills, but to certain small vacuities possessed by the embryo of fish in its early states. There is therefore nothing but a mere general analogy in the development of the vertebrata. All analogy ceases whenever we attempt a comparison between the fœtus of the latter with the perfect avertebrata. They have no range of ganglia along the abdominal aspect of the body, but have a spinal cord. The circulation is, from the very commencement, totally different from that of the mollusca, crustacea, &c.; and the organs of sense are, from their first beginnings, quite dissimilar *.

But some have gone farther, and maintain, that the higher organs are nothing else than repetitions of the lower organs of the same organism. From which has arisen the doctrine of the "Relative Importance of Equivalents," &c., which have afforded scope enough to the imagination, but have contributed but little to the extension of our acquaintance with the essence of life.

In my opinion, the following principles must be laid down as the base of every inquiry into the relations between different living bodies, with respect either to the whole of their organization or their individual organs.

1. The rank of a living being is higher the more numerous are its points of contact with the external world, the more di-

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* Many other insuperable objections against the doctrine of the development of organic bodies upon one prototype have been brought forward by *Baer*, in his *Beiträgen zur Kenntniss der niedern Thiere* (Verhandl. der Kaiserl Acad. der Naturforsch. xiii. Abth. 2. p. 739, &c.), and in his *Entwickelungsgeschichte der Thiere*, Th. i. p. 199. *Weber* has also very clearly expressed himself on the point in his edition of *Hildebrandt's Handbuch der Anatomie des Menschen* (i. 125).

versified its excitability. The number of these points of contact increase with the advancement of the intelligence, which necessitates a proportionate increase in the variety and perfection of the several organs. The intellectual faculty is more perfect in man than in any other terrestrial existence. He, therefore, merely in an organic point of view, stands at the head of the scale of animals. But this does not oblige us to suppose that in him every organ has reached the maximum of its development. An important line of distinction must here be drawn between the organic sphere of the sensitive and of the simply organic life. With respect to the former class of organs man is decidedly at the head of all animals, but by no means in respect to the latter. His alimentary apparatus, for instance, is not nearly so complicated as in many other animals. Between him and the lowest existences there are many members, of which one cannot be said to occupy a higher place than another. One being is best adapted for one purpose, and another for a different purpose. They can only be placed in a linear series, by arranging them according to the type of some one of their organs.

2. An original form may be very easily supposed, out of which all living things are developed. This development is not in one but in several directions. Each of these principal directions again give rise to new subdivisions, so that the whole assumes somewhat of an arborescent form. Different twigs, however, of one of the branches often unite those of another higher or lower in the scale. In the midst of all these diversities, a general similarity persists between the various organisms and their several parts, which enables us to trace them up to a common fundamental form. No conclusions, however, can be drawn from the similarity of two forms, regarding their respective superiority or inferiority in point of development. Such similarity may result from the lateral concurrence of two forms derived from totally different twigs. Insects, for example, resemble the vertebrata in some of their organs, but are otherwise so different from them in all their parts, that this partial exception cannot be considered to result from a higher development of the form peculiar to insects. As in the higher animals, so we can likewise distinguish in the alimentary canal of insects, a se-

paration into œsophagus, stomach and intestine. The latter, moreover, is not as in the former attached to a liver. Its posterior extremity certainly opens as in them above the generative organs. But their proper tongue is not situated below but above the entrance of the pharynx. When, however, we speak of development from a common primitive form, and the combination of one form with another, it is not to be understood that these developments and combinations follow the same rules as pairing and generation in the presently existing nature.

3. No conclusion can be drawn from the absence of an organ, regarding its purpose, nor *vice versa*. An example of this is furnished by the auditory ossicula of fish, discovered by Weber, which are evidently parts of the vertebral column, although attached to the organ of hearing, which in other animals is quite unconnected with the column. Another instance is in the mandibules of the crustacea and insects. These organs have the same function as the maxillæ of the vertebrata. But they do not like the latter move in a plane parallel to the axis of the body, but in one at right angles to this axis. In many crustacea, moreover, they perform the function of legs. On the contrary, the organs of these two classes which move in a plane similar to that of the jaws of the higher animals, viz. the upper and under lips, have a totally different function.

To return to our proper subject, the distribution of living existences; according to our principles of considering life and the intellectual faculties as one and the same, we may be allowed to consider the organs through the medium of which the activity of the living principle passes to the other bodies in the animal kingdom, as those with whose perfection the structure of the other parts must stand in connection. And the more intimate the connection the higher the gradation of life. These organs are the brain, spinal cord, and nerves.

The presence or absence of these organs, as characterising the existence or non-existence of voluntary motion, has always been considered as the basis of the two grand divisions of living nature into animals and plants. No nerves, it is true, can be detected in the simpler animals; but as they have been discovered in some to which they were formerly refused, and as these species are intimately connected with those in which they have not yet been discovered, it is not improbable that they are an attribute of all animals, even the lowest. It is quite otherwise with plants. Their tissues are so transparent, that if a nervous matter at all analogous to that of the nervous animals existed, it would long ago have been observed.

Here is, therefore, a negative character of plants; and in their classification some other system than the nervous must be made the basis of division. Their positive character consists in the composition of their whole internal substance of solid cells and unbranched vessels. Their cells are separated by intervals, and are connected together in a reticulated form. These intervals have no proper coats. Globules and not cells are the elementary parts of the animal tissues, and their vessels are always branched. When we approach the confines of the animal and vegetable kingdoms,—whenever the vegetable tissues are destitute of solid cells and of vessels, it is always difficult to determine to which of the two kingdoms they ought to be referred, from their want of the distinctive characters of the animal and vegetable tissues.

It might be supposed that the embryo, with its envelopes, was the miniature expression,—the microcosm of the perfectly developed animal, and that the complexity of each were directly proportioned to each other, so that the degree of affinity between organic existences might be concluded from the similarity of their ova and embryos. The appreciation of these resemblances has great or even insurmountable difficulties in animals, but is much easier in plants. The characters deduced from the formation of the embryo and its envelopes, have been found the fittest for the natural arrangement of plants. They are the bases of the Jussieuan System, which I must suppose to be well known to all.

Rudolphi was the first who attempted to arrange animals according to the structure of their nervous system*. He divided them into those with, and those without, a visible nervous system; but such a division is of no value for biology. Our author has only given the most general distinctions of the classes provided with a nervous system. I have also occupied myself much with this subject; but my contributions are still merely fragments; but I doubt not that a very splendid superstructure might be raised, if the principles were applied to the filling up of all the gaps.

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* Beiträge zur Anthropologie und Allgemeinen Naturgeschichte.

According to my view, there are two great divisions of the animal kingdom. The one consists of those provided with a true spinal cord, inclosed within a shut vertebral column, which is always wanting in the other. In the former, the whole brain is situated above the pharynx, and inclosed in an osseous capsule, the skull. In the other there is either a single cerebriform mass, or several nervous knots united together by cords of nervous substance surrounding the œsophagus. The mass analogous to the brain is sometimes above, sometimes below this canal, and is developed in an osseous or horny capsule, which separates it from the other parts of the head; and its posterior part gives out no prolongation comparable with the spinal cord of the other animals. This division, therefore, coincides with that of Lamarck into *Vertebrata* and *Avertebrata*. They might also be called *Cranial* and *Acranial* animals. We shall, however, adhere to the old epithets of Lamarck, to avoid overloading biology by the introduction of new names.

In all the vertebrata, the anterior extremity of the spinal cord inclosed in the cranium, possesses the same form that it does in man; and in all undergoes a considerable augmentation both in mass and volume, compared with the rest of the cord. Even from the numerical differences of this proportion, four classes can be distinguished in these animals. I found the limits of the proportional weights of the spinal cord to the brain to be,—

In the first class from	1 : 85 to 1 : 6.5
second,	1 : 24.3 to 1 : 6.7
third,	1 : 3.6 to 1 : 3 2
fourth,	1 : 3.5 to 1 : 1.0

The relation of the greatest breadth of the cord to the greatest breadth of the brain ranges,—

In the first class, from	1 : 6.85 to 1 : 1.20
second,	1 : 4.45 to 1 : 1.12
third,	1 : 2.55 to 1 : 1.35
fourth,	1 : 1.43 to 1 : 1.28

The first class comprehends the mammalia, including man; the second, birds; the third, reptiles; the fourth, fish.

The following are the most important of the other neurological differences of these classes:

In the mammalia, the portions from which the olfactory and optic nerves take their origin, coalesce with the rest of the cerebrum, no limits being distinguishable between them. In all the other vertebrata, the cerebrum is distinguished into anterior and posterior hemispheres, from the former of which arise the olfactory, and from the latter the optic nerves. The posterior hemispheres may be either separate or united together in the mesial line. They are separate in birds, united in reptiles and fishes. Moreover, these posterior hemispheres may be smaller or larger than the anterior,—smaller in birds, reptiles, rays, and sharks; larger in other fishes.

The mammalia possess a cerebellum, consisting of a middle portion (the vermiform process) and two hemispheres, and its vertical section displays an arborescent prolongation of the cord (arbor vitæ). In birds the hemispheres of the cerebellum are merely rudimentary; the vermiform process, however, still contains a distinct arbor vitæ. This last is entirely wanting in reptiles and fishes, and the cerebellum consists of a simple sack, formed by a thin layer of the cord.

The mammalia alone possess the pons varolii (or great cerebral ganglion).

In fishes, there are found at the base of the brain, behind the optic tracts, two symmetrical eminences, of such extent that they form the greater part of the base, and do not much yield in size to the posterior hemispheres.

The avertebrata, in the same way, fall under two great divisions, according to differences in the structure of their nervous system. In the one, there is extended along the abdomen, in two rows, or in the mesial line, a series of ganglia united to one another, and to the middle of the under half of the cerebriform ring by nervous filaments, and the upper part of the ring consists of two hemispheres, immediately united together. In the other division, there is no series of ganglia extending in regular order the whole length of the animal, and the two lateral tuberosities of the ring are not immediately united, but by means of bands of nervous substance. To the first division belong those animals whose bodies consist of articulated rings, comprising the crustacea, insects, and worms; to the second, the molluscous animals and zoophytes.

The distinguishing neurological mark between the crustacea and insects, and the worms, is the inequality in the several portions of the abdominal ganglia. This is obvious in the higher insects at the first glance. Their thoracic ganglia always differ in form and size from the abdominal. The difference is not so marked in the genera nearest to the worms,—the *Millipes*, *Scolopendrum*, and *Julus*. But even here the ganglia, from which arise the nerves supplying the organs of generation, are different in size from the others; whereas in the worms, no other distinction is observable between the ganglia than a general decrease in size and relative distance towards the posterior end of the animal. They have, in common with most crustacea and insects, the possession of but one series of ganglia situated in the middle line of the abdomen, and composed of a symmetrical right and left half. A deviation from this structure exists in the phalangiæ which possess ganglia on both sides of the abdomen, but united with one another only by simple filaments.

This first division passes into the second by the cirrhipedes, which, in the articulated posterior part of their bodies, possess a series of ganglia, as in the former animals, but in the upper portion of their cerebral ring, no hemispheres immediately connected together. Among the mollusca, there are families distinguished from all other animals by the want of symmetry of their brain. In those where the cerebral ring is symmetric, it has no central mass. Nothing certain is known of the nervous system of the zoophytes. But from the general radiated structure of this class of animals, it is probable that it is merely a simple cord, surrounding the orifice of the alimentary canal, or an aggregate of ganglia united in the form of a ring, and which give off nerves in a radiated manner.

Much is still wanting to carry out this neurological classification in all its details; and even did we possess an adequate mass of observations, a more uniform division could hardly be proposed. It will, however, be by no means out of place to trace the concordance of the characters deduced from the nervous system, with the natural characters of the families for the class of mammalia.

The proportion of the spinal cord to the encephalon which we have already found of such value in the primary divisions of the vertebrata, is no less important in their further subdivision. I have found it to correspond with, and to follow, analogous gradations, with the natural characters in the genera and species. Of these dimensions, the most important are the greatest breadths of the two organs, which agrees with their relative weights, and very nearly with the transverse diameters of the cranial cavities, and of the occipital foramen. In the following table, I have brought together several of these relations, and have ranged the species according to their natural affinities. Those marked with an asterisk are taken from *Tiedemann's Icones Cerebri Simiarum et quorundam Mammalium Rariorum* ; the others are from my own observation. Most of these last depend on immediate measurements of the brain and cord. Those marked with a cross, are the relative breadths of the occipital foramen and of the cranial cavity.

The greatest breadth of the spinal cord immediately behind the pons is to the greatest breadth of the cerebrum,

	In Man, as	100 : 685
	Simia sphinx,	: 470
	capucina,	: 430
*	Sabæa,	: 414
*	Resus,	: 365
*	Cynomolgus,	: 352 to 360.
*	Nemestrina,	: 357
*	Lemur Mongoz,	: 371
†	Ursus maritimus,	: 322
	Canis vulpes,	: 278
	Mustela Foina,	: 270 to 308
*	Felis Leo,	: 275
*	Nasua narica,	: 208 to 248
*	Lotor vulgaris,	: 248
	Erinaceus europæus,	: 230
	Talpa europæa,	: 200 to 265
	Vespertilio myotis,	: 192
	Didelphis virginiana,	: 147
*	Mus alpinus,	: 227
	Rattus,	: 204
	Cricetus,	: 205
*	Hystrix cristata,	: 200
*	Castor Fiber,	: 207
*	Cavia Agouti,	: 222
	Lepus timidus,	: 230
	Capra ovis,	: 250
	Sus Scrofa sinensis,	: 222
*	Phoca vitulina,	: 352
†	*Monodon* Narwhal,	: 363

We have here a double series, an increasing and a decreasing. The first ranges from man to the Virginian Opossum; the second continues from the Opossum to the Cetacea. The intermediate members of the first series are, first, the apes, then the makis (Lemur), then the plantigrade or digitigrade carnivora, and, lastly, the insectivorous genera of Erinaceus, Talpa, and Vespertilio. In the second series, the links are filled up by the Rodentia, of which the hare forms the passage to the Ruminantia; to which follows the Pachydermata, which pass through the Phocæ into the Cetacea. Thus the relations of the spinal cord to the brain in these animals, entirely coincide with their other natural affinities. Some writers, it is true, have regarded their affinities differently, making the bat succeed to the makis, and the phoca to the plantigrades. But, in the determination of these affinities, we must not be swayed by a few of the more obvious external marks, but by the whole internal as well as external conformation. Upon such a basis these animals will assume the order that we have given them above. The only analogy of the bats with the makis is in the pectoral mammæ; and the phocæ are intimately unconnected with the cetacea through the genus Manatus.

By this, however, I am far from asserting the mammalia can be ranged in an uninterrupted series, according to the proportions of the brain to the cord. Even in the above list, deviations are found from such an order. This is owing partly to the relative breadths of these two organs not being exactly indicative of their relative masses; and to the circumstance of the number of individuals in which the masses are determined with precision being still too small to justify the formation of an exact series. Only, if these defects were supplied, it might be expected that, in order to preserve the scale of natural affinities, the vacuities of the ascending and descending series should be filled up. It cannot be doubted, for example, that, in the sloths (bradypus), the brain, compared with the cord, is smaller than in the makis and the uppermost of the carnivora. They are, however, immediately connected with the makis, and must be placed in the series between the lemur and ursus. If the proportional breadths of the organs were as 100 to 300, the descending series from man to the opossum would change into two others, one from man to the sloth, the other from the bear to the opossum. Farther inquiries would likewise disclose other intermediate series. Such interruptions of the principal series are, however, quite correspondent to the laws of natural affinity. They would only transgress these if interposed members gave rise to recurrent series in one of the two principal; if, for instance, an animal which, according to its natural affinities, succeeds to the sloth, should, from the proportion of its brain and spinal cord, stand above it. But I do not believe that such a genus can be found.

As every genus and species, not only of the mammalia, but also of the other vertebrated animals, has a certain proportion between its brain and spinal cord, in the same manner each part of the brain, especially the cerebellum, bears a definite proportion to the other parts. Hence we might deduce further specific and generic marks; and one day we may be able to distinguish the whole of the vertebrata by the numerical masses and dimensions of the parts of the encephalon, and range them from this character according to their natural affinities. The relation, it is true, varies in different individuals of each species, but there are limits which it never exceeds.

Analysis of the Labradorite Felspar found in the Trap-Rocks of Scotland. By Captain LE HUNTE. Communicated by the Author.

Silica,	54.674
Alumina,	27.889
Lime,	10.600
Soda,	5.050
Potash,	0.490
Magnesia,	0.181
Protox. of Iron,	0.309
	99.193

The specimen analyzed was found near Campsie, in a porphyritic greenstone. It was in the form of long narrow crystals, nearly transparent and colourless, of a foliated structure, and vitreous lustre. Its specific gravity was 2,689. The larger crystals frequently present a flat conchoidal fracture, which first induced me to suspect that it was not felspar. From the great difficulty that I experienced in procuring the mineral in a pure state, the analysis was made upon a small scale; but it was repeated with nearly the same result. It appears, then, that this mineral is labradorite, with the best analysis of which the foregoing nearly agrees. The large crystals of labradorite that are imbedded in trap-rocks are very much cracked, and so impure that they cannot be employed for analysis.

About two miles to the west of the village of Milngavie, near the road between Glasgow and Strathblane, there is a very remarkable brown porphyritic trap, that contains large and beautiful crystals of yellow labradorite. These crystals are cracked, and when heated, present numerous brown spots, which show that they are not pure. The following is their composition:—

Silica, ...	52.341
Alumina, ...	29.968
Lime, ...	12.103
Soda, ...	3.974
Potash, ...	0.301
Perox. of Iron, . . .	0.866
	99.553

All the analyses of labradorite that have hitherto been made differ a little from each other; some agree with my first analysis, while one of Klaproth's differs but little from the last. It is probable, that, owing to its structure, labradorite has seldom been examined in a pure state.

The chemical characters of labradorite enable us to distinguish it from felspar, even when the quantity for examination does not exceed a grain. For this purpose the mineral, in the state of an impalpable powder, is treated with weak muriatic acid in a watch glass, and gently heated for an hour. The solution is then evaporated to dryness, and the saline residue is heated until the excess of acid be expelled when it is redissolved in water. To the clear solution, when warm, a few drops of oxalate of ammonia are added, which produces a precipitate of oxalate of lime if the mineral be labradorite, but does not produce a precipitate if it be pure felspar.

Dr Macculloch mentions, that the glassy felspar which he found at Sky frequently resembled labradorite. I have seen many specimens from that island to which the latter name must be applied. The mineral, which gives a porphyritic aspect to some of the pitchstone of Arran, appears to be glassy felspar; as far, at least, as I can judge, from having examined a very small fragment of it. The porphyroidal traps, on the western boundary of the Scotch coal district, more frequently owe their structure to the presence of labradorite than to that of felspar. This last mineral, with its usual characters, very rarely occurs in them. We frequently indeed find a white, opaque, foliated mineral, destitute of lustre, which has generally been called felspar; but I have procured both lime and soda from this substance; therefore it has, in some cases at least, been improperly named. The trap-rocks appear to contain other minerals which resemble felspar in many of their characters, but differ from it in composition. One of these, which I found in the neighbourhood of Stirling, is of a red bright colour, opaque, foliated, and has a silky lustre; from the small quantity of it which I could procure I could only determine, that it contained a good deal of peroxide of iron, and a little lime. The analysis of another was published some time ago by Dr Thomson, who called it Mornite. It is very remarkable that the composition of mornite is precisely the same as that of the first specimen of labradorite that I examined, excepting that it does not contain an alkali, but it contains a quantity of protoxide of iron, exactly equivalent to the soda in labradorite. The external characters of mornite and labradorite must also be very similar, for the mineral dealer from whom Dr Thomson procured the former, when I showed him my specimens of the latter, immediately called them mornite. I have not seen this mineral, which was found in a trap-rock in the north of Ireland. The granitoidal traps of the Scotch coal district, on the contrary, very frequently present well characterised crystals of felspar, and rarely labradorite. They often, however, contain a white mineral, which, through a great extent of rock, does not present any of the characters of felspar, but rather resembles a zeolite; and these differ very much, in all their characters, from those which contain felspar. It would be easy to arrange the granitoids, from their external characters, into three classes, and to give them very expressive names, if the minerals which they contain were well known. The unicoloured crystalline traps occasionally contain labradorite, and the presence of this mineral may account for the soda that has generally been found in them, as well as in basalt, to which they are closely allied; but it does not, in some cases, account for the whole of the soda, so that they probably contain other minerals, of which this alkali forms a constituent part. I have analyzed two of these rocks, that contained labradorite, and the analysis differed but little from those of some basalts. The neighbourhood of Glasgow affords some very fine specimens of columnar trap, which all contain labradorite. The range of large and regular columns that stands on the side of a wooded hill, a little to the south of Strathblane, is well known. It is composed of a dark green trap or greenstone, containing crystals of that mineral. The same may be said of the black basalt, that forms the remarkable horizontal columns, near Altmarry turnpike, on the road from Glasgow to Drymen; and also of the columns that are found in more than one place on the Kilpatrick and Cathkin Hills. So that labradorite is probably one of the minerals of which basalt is often composed.

I have made these remarks upon the situations in which I have found labradorite, because I believe that we cannot attempt to arrange the trap-rocks, or give them expressive names, until we are better acquainted with their composition. The present nomenclature of these rocks is very defective, and, not being formed upon any fixed principle, it cannot be improved. The names must either be multiplied to such an extent as to render them very burdensome to the memory, or be applied so loosely, as always to require a definition. The short and simple terms now in use have not the flexibility which names, applied to objects that are constantly varying in their characters, should possess. It would be better to employ in geology, as often as possible, compound descriptive names, that would indicate the structure and composition of rocks. Such names would, in many cases, be shorter than those now employed, as they never would require a definition; and, when formed upon a fixed principle, they might be multiplied without loading the memory. As far as the trap-rocks are concerned, they can be accurately named in this manner only; for we frequently find among them rocks of the same composition, but differing in structure; and, on the contrary, their structure is often the same, when their composition is different. They are easily arranged in a few classes, by their external characters; and the class to which the rock belongs may be expressed in a single word, while its composition may be denoted by two or three. There are some cases in which we should still be obliged to use arbitrary terms; but this should not induce us to reject the only principle upon which an expressive and convenient nomenclature can be formed for the greater number of these rocks. I have long been in the habit of using those compound terms in a district which presents trap under a great variety of forms; and I should mention the mode that I have adopted, to render them as short and expressive as possible, but that my observations and experiments on the subject are far from being completed.

Physiological Investigations arising from the Mechanical Effects of Atmospherical Pressure on the Animal Frame. By JOHN DALTON, F. R. S.

A PERIOD of a century and a half has elapsed since the inventions of the barometer and the air-pump. In this time the weight of the atmosphere, its elasticity, its specific gravity, and many other properties, have been ascertained experimentally with almost mathematical precision. The weight of the atmosphere, that quality we have more particularly to consider in the present essay, is not constantly the same, as is proved by the rising and falling of the barometer. It varies in this part of the earth from $\frac{1}{12}$th to $\frac{1}{15}$th of the whole weight at certain times; but those variations are gradual, so that it requires some days or weeks before the weight passes from one extreme to the other. On an average the weight or pressure of the atmosphere amounts to 14½ lb. on each square inch of surface of the earth; and, as fluids press equally in all directions, every square inch of surface, whatever may be its position, must be subject to the same pressure. The surface of the human body, as well as that of animals in general, has to sustain this pressure; and it will be found by calculation, that the whole surface of a middle-sized person, will have to support from 15 to 20 tons of pressure all acting inwards, and having no other mechanical tendency than that of squeezing or compressing the materials of which the body is composed into a less compass.

The above is a statement of facts, all of which I believe to be incontrovertible. But a very difficult question arises out of them, How is it that the animal frame is utterly insensible of the whole, or of any part of this enormous pressure upon it? In ordinary we feel no pressure on the surface of our bodies, either external or internal; neither when the barometer is stationary nor when it is in a most fluctuating state. I have never met with a satisfactory answer to this question, and I doubt whether such a one has ever been given; yet it must be allowed to be one of importance, both as it affects the physiology of the animal and vegetable kingdoms. Having had occasion for a few weeks past to ruminate on this subject, some new views have occurred to me; and it is the object of the present essay to unfold them, in order to elucidate the phenomena arising from aerial pressure on the animal economy more especially.

It is pretty well known that the specific gravity of living men in general, is less than that of water. Mr Robertson, formerly librarian to the Royal Society, procured an apparatus for the purpose of determining the specific gravity of the human body. He chose ten men promiscuously for the purpose. Of these, three were found very nearly of the same weight as water, one being a little heavier, and the other two a little lighter than water; two others were found only about .8 the weight of water; but the other five were of intermediate specific gravities. The average of the ten was, height 5 feet 6⅔ inches; weight, 146 lbs.; specific gravity, 891; bulk, 2.618 cubic feet. From this I think we may safely infer that the body of a full grown living man, when plunged over head in water, will be found upon the average to be nearly .9, the weight of an equal bulk of water.

It is remarkable that all the component parts of the animal frame, at least of the human subject, are severally specifically heavier than the whole body, with the exception of air. Bone, muscular flesh, blood, membrane, &c. are all heavier than water;

animal fat is perhaps the lightest of the components, but even this is heavier specifically than the whole man upon the average. Bone from the leg of a calf I found to be 1.24 specific gravity. The lean of beef (raw) I found 1.045 specific gravity. Blood is from 1.03 to 1.05 specific gravity according to circumstances. On the whole, the solid and liquid parts of the body, examined after life is extinct, would appear on an average to be somewhere about 5 per cent. heavier than water.

That part of the volume of man which is exclusively occupied by air, and which may therefore be considered as adding nothing materially to the weight of the body, consists of the air-tubes and air-cells of the lungs, the trachea or windpipe, the mouth and other appendages. It is not easy to ascertain the medium volume of air in the lungs of any individual. Messrs Allen and Pepys found the air remaining in the lungs of a man after death somewhat exceeded 100 cubic inches. I found formerly that after a full inspiration I could blow out 200 cubic inches of air from my lungs, but was then quite exhausted. My ordinary inspirations and expirations amounted each to about 30 cubic inches *.

Judging from the above facts and considerations, I should be disposed to conclude that the medium volume of air in the lungs of a middle-sized person would not be less, but rather more, than 100 cubic inches. Besides the lungs there are no other receptacles for air, I believe, in the body except the stomach and bowels, which are occasionally more or less inflated with portions of air either from the atmosphere or other sources. If we allow 150 cubic inches for the volume of air contained in the whole man when entirely immersed in water, it will be as fair an estimate, perhaps, as can be made. But it may be imagined by some that the whole substance of the body is pervious to air; that the skin, the flesh, the blood and even the bones, may be imbued with air, somewhat in the same manner that water is, and yet have no cavities or cells in which the air is collected into a visible volume. Whether such an idea has ever been entertained or discussed I am not aware; but I presume no one has succeeded in determining either the nature or the quantity of the air so enveloped in the system. We shall

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* Memoirs, vol. ii. (New Series, p. 26.)

now examine how far such a notion is countenanced by the preceding statement of facts.

According to the preceding table of Robertson, the average bulk of the ten men was 2.618 cubic feet, 4.500 cubic inches nearly; but of this volume 150 inches according to the above estimate were air, and the remainder 4350 inches were solid and liquid parts of the body. Now the average specific gravity of those parts of the body has been estimated above at 1.05 when examined as dead matter: this would make their weight equal to 4567 cubic inches of water; whereas it was found by actual weighing, to be 146 lbs. as per table, = 4044 cubic inches: hence the observed weight was less than the calculated weight, a portion equal to the weight of 523 cubic inches of water, or more than one-ninth of the whole weight of the body.

Here is a discrepance that demands an investigation. Can Robertson's table of the specific gravities of men give too low an estimate? This is not likely; every one knows that the human subject generally floats in water till the lungs become filled with that element,—a proof that the body is lighter than water; and many persons are observed to swim with the whole head constantly above the surface of the water.

Have we overrated the specific gravities of the component parts of the body? I think not: bones, and flesh, and blood are certainly all heavier than water, some more, some less.

Has the capacity of the lungs for air been underrated? I cannot imagine that any one will contend that the lungs of a middle-sized man will hold, at a medium state of inflation, six times the volume of air we have assigned. Upon the whole, I am inclined to believe the true explanation of the difficulty will be found in this, that the substance of the body is pervious to air, and that a considerable portion of it constantly exists in the body during life, subject to increase and diminution according to the pressure of the atmosphere; in the same manner as it exists in water: and, further, that when life is extinct, this air in some degree escapes and renders the parts specifically heavier than when the vital functions were in a state of activity.

The facts that water absorbs air of all kinds, that the quantity of the air absorbed is proportioned to the pressure and density of the gas, whether it be alone or mixed with other gases, and that certain laws of equilibrium take place, by which water acquires that state in which it is disposed neither to give out nor to take in any more gas, have been abundantly proved by Dr Henry and myself. M. Saussure has shown the like for other liquids, and for a great number of solid bodies. It may be seen, too, in my Chemistry, vol. i. p. 236, that a bladder, which is generally considered as an animal membrane, least pervious to air, may be filled with one gas, and being some time exposed to the atmosphere, it will be found to continue full blown as at first, but the contents will be chiefly atmospheric air. Messrs Allen and Pepys, in their ingenious and excellent essays on respiration, have proved that when a Guinea pig or a pigeon is confined for an hour, more or less, in a mixture of hydrogen and oxygen gases, in proportion as 78 to 22, a large portion of azotic gas is found in the residue, and an equal portion of hydrogen disappears. They ascribe this change to effects of respiration, but it appears to me more probably due to the principle we are advocating; namely, to the egress of azotic from the whole body, and the ingress of hydrogen in lieu of it, in consequence of withdrawing the external pressure of the former and substituting that of the latter.

When the palm of the hand is placed over the top of the receiver of an air-pump, and the air is exhausted, the pressure of the air on the outside is scarcely felt, but the inside is swollen and feels as if it was drawn or sucked into the receiver. Thus the sensation is on the inside and not without; but there is within, and the consequence is a tendency of the air in the hand to escape into the receiver, which occasions the pain and swelling. It is thus also that the issuing of blood in the surgical operation of cupping is effected.

Though it does not seem of much consequence what the pressure of the air may be on the animal frame within certain limits, yet sudden changes must always be accompanied with uneasy sensation. Climbing mountains, or ascending in a balloon, removes a part of the atmospheric pressure from the body; this causes the air in the body to tend outwards, and sometimes occasions bleedings. To supply oxygen to the lungs, a greater volume of air must be breathed, and this seems to produce an acceleration of the pulse. On the other hand, by descending

30 or 40 feet deep into the water in a diving bell, the pressure of the air upon the body is increased inwards; pains in the ears are felt from the difficulty of suddenly restoring a disturbed equilibrium; but if the descent is slow and interrupted, time is given for the air to enter the pores, and the pain is less sensible. To what limit warm-blooded animals could bear rarefaction of air so as to subsist, has not, that I am aware of, been determined with much precision. Ascents in balloons have been made till the atmospheric pressure was reduced more than one-half. Formerly I found that a mouse could subsist in $\frac{1}{4}$th of atmospheric density and seemed not to have suffered much; but upon reducing the density below $\frac{1}{4}$th, the animal was convulsed and expired immediately, notwithstanding the air was instantly admitted.

If the view we have expounded in this essay, in regard to the action of aerial pressure on the animal frame, be correct, it may be inferred, that the pressure admits of great latitude; perhaps an animal could subsist under the pressure of half an atmosphere, or of three or four, or more atmospheres. The uneasiness and danger would be found in the quick transition; if time is allowed for the air to enter the body, and to escape from it, the transition is gradual, and the sensation arising from it imperceptible. The animal economy would be adapted to it, like as in the transition from a cold to a warm climate. It may hereafter be found, what length of time is sufficient to adjust the equilibrium, and whether this subject is any way connected with certain diseased states of the body. As far as regards the absolute pressure on the body, and our insensibility of it generally, this question will be met by the argument, that the air within the body, by its elasticity, sustains a corresponding pressure from without; but this only accounts for our alleviation from a small fractional part of the whole exterior pressure. The greater part must still be supported by the body; and we must have recourse to the great incompressibility of matter to account for our insensibility of pressure. Canton found that water, pressed by one atmosphere more than ordinary, only exhibited a reduction of $\frac{1}{21740}$th part of the whole; if the same rate, applied to the compression of the human body, the reduction or compression of the size of a man, 4500 cubic inches, would only be $\frac{1}{5}$th of a cubic inch, for the weight of an additional atmosphere. Now as the body consists of solids and liquids of almost incompressible matter, and there is only a small part of the volume consisting of elastic fluid that is compressible, no material change of volume can take place, but on the sudden transition from one atmospheric pressure to another; and unless a change of volume take place, we cannot feel any pressure, either inward or outward. The phenomena of the water hammer shew, that the particles of water are hard, as they strike each other like flint and steel; and it is exceedingly probable that other bodies, solids as well as liquids, are constituted in like manner. A general pressure on the system, then, only increases in a small degree the attraction of the ultimate particles, and it is met by a corresponding increase of repulsion from the atmosphere of heat; so that the system remains as nearly as possible the same, and unaffected by such pressure.

I can scarcely forbear observing on the present occasion the absurdity of those who remark, that all people might swim, and that it is only from fear or ignorance of the art that some fail in the attempt. When we see that some persons are heavier than water, and others only .8 of that weight, it would be just as plausible for a piece of deal to upbraid a piece of lignum vitæ with the inability to swim from fear, or from want of skill in the art, which the deal considered of easy acquisition.—*Manchester Memoirs*, vol. v. New Series.

Chemical Analyses of Spinel, Gahnite, and Chrome Ore.

By Mr HERMANN ABICH.

Constituent Parts.	Blue Spinel, Sweden.	Red Spinel, Ceylon.	Pleonaste, Ural.	Pleonaste, Vesuvius.	Gahnite, Sweden.	Gahnite, America.	Massive Chrome Ore.	Crysta Chro Or
Silica, ...	2.25	2.02	2.5	2.38	3.84	1.22	0.83	00.
Alumina, ...	68.94	69.01	65.27	67.46	55.14	57.09	13.85	11.
Oxidulated Chrome,	00.00	1.10	00.00	00.00	00.00	00.00	54.91	60.
Magnesia, ...	25.72	26.21	17.58	26.94	5.25	2.22	9.69	7.
Oxidulated Iron, .	3.49	0.71	13.97	5.06	5.85	4.55	18.97	20.
Zinc-Oxide, . . .	...	...	...	...	30.02	34.80	...	..
Oxidul. Manganese,	Trace	00.00	...	...	Trace	...	...	..
Sum, .	100.40	99.05	99.32	100.84	100.10	99.38	98.25	99.

On the Uniform Permeability of all known Substances to the Magnetic Influence, and the Application of the fact in Engineering and Mining, for the Determination of the Thickness of Solid Substances not otherwise Measurable. By the Rev. WILLIAM SCORESBY, F. R. S. Lond. & Edin., Correspondent of the Institute of France, &c. &c. Communicated by the Author. Concluded from p. 334 of precrding Vol.

INVESTIGATION OF THE LAW OF THE DIRECTIVE POWER OF BAR-MAGNETS, AND THE EXTENT TO WHICH THE MAGNETIC INFLUENCE MAY BE CARRIED FOR MEASURING DISTANCES.

2. THE *law of the directive power of bar-magnets, at different distances*, was the next subject of investigation.

Coulomb, I believe, was the first to establish, by the test of satisfactory and consistent experiments, the previously assumed law, that the force of magnetic attraction and repulsion is in the inverse ratio of the squares of the distance. The application of this law to the investigation in hand served at once to verify the law, and to render the results of my experiments of general application. In regard to the comparison of distances, it appeared to me to be of considerable advantage to estimate all distances in *lengths* of the bar made use of, by which the results for any one bar became applicable to all other bars of a proportional form and quality, and state of magnetic energy. And such, therefore, with a certain modification, afterwards found to be necessary, was the measure constantly adopted.

Placing now the magnet in the direction of the east or west point of the compass, or at right angles to the magnetic meridian, I proceeded to ascertain experimentally the deviations produced, either by the same pole constantly, or by the mean of the action of each pole alternately, first at the distance of one length, and then successively at other distances to the extent of ten lengths of the bar.

Preparatory, however, to a general application of the results thus obtained, it will be useful to ascertain by calculation the actual force exerted by any magnet on a compass at different distances, according to the above law of attractions.

When the bar is placed in the prescribed position, with the north pole at the distance of *one* length from the compass, then the action of the south pole, tending to counteract that of the north, will be in the inverse relation with the nearest pole of 2^2 to 1^2. That is, if the force of the nearest pole be called 1, then the force of the remote pole will be inversely as 4 or $\frac{1}{4}$th, which being in the contrary direction to that of the nearest pole, reduces its action to $\frac{3}{4}$ths. In like manner, at *two* lengths of the bar, the force of the nearest pole being now inversely as the square of 2 or $\frac{1}{4}$th is reduced by the remote pole at three lengths distance, the action of which is inversely as the square of 3 or $\frac{1}{9}$th; hence $\frac{1}{4} - \frac{1}{9} = \frac{5}{36}$, representing the actual influence or general resultant of the whole of the magnetic forces in the magnet acting upon the compass.

But we may obtain a general expression for all distances, either in lengths or fractions of lengths of the bar.

Let F be the influence of the nearer pole at the distance a.

Then will $\frac{F a^2}{x^2}$ represent the influence at the distance x, and $\frac{F a^2}{\overline{x+1}^2}$, *the counteracting influence* of the remote pole at the distance $\overline{x+1}$.

Hence the *resultant influence* is,

$$F a^2 \left(\frac{1}{x^2} - \frac{1}{\overline{x+1}^2}\right) = F a^2 \frac{\overline{x+1}^2 - x^2}{x^2 \,.\, \overline{x+1}^2} = F a^2 \frac{2x+1}{x^2 \,.\, \overline{x+1}^2}.$$

As the force F, however, being the separate action of a pole not practicably separable, is not a quantity that can be immediately ascertained by experiment, this expression requires to be extended so as to connect it with the value of the assumed force :—

Let R be the resultant influence of a magnet on the compass at distance a, and, in the first instance, let a be equal to x.

$$\text{Then} \quad R = F a^2 \frac{2a+1}{a^2 \,.\, \overline{a+1}^2} = F \frac{2a+1}{\overline{a+1}^2}$$

$$\text{Therefore} \quad F = R \frac{\overline{a+1}^2}{2a+1} : \text{ Hence,}$$

$$F a^2 \frac{2x+1}{x^2 \,.\, \overline{x+1}^2} = R \frac{a^2 \, \overline{a+1}^2}{2a+1} \,.\, \frac{2x+1}{x^2 \,.\, \overline{x+1}^2}.$$

The nature of the forces acting upon the needle, at various distances, with the general results, and a reduction of these results to other ratios, are exhibited in the following Table.

Distance in Lengths.	Comparative Influence of each Pole.	Relation of the Forces acting on the Compass.	General resultant or actual Comp. influence. $F a^2 . \frac{2x+1}{x^2 . \overline{x+1}^2}$	Ratio of resultant reduced	
				To the relation R being = 1.	To the relation of 1.0 inversely.
1	$\left.\begin{matrix} n = \frac{1}{1} \\ s = \frac{1}{4} \end{matrix}\right\}$	$= \frac{1}{1} - \frac{1}{4}$	$\frac{3}{4}$	$\frac{1}{1}$	1.0
2	$\left.\begin{matrix} n = \frac{1}{4} \\ s = \frac{1}{9} \end{matrix}\right\}$	$= \frac{1}{4} - \frac{1}{9}$	$\frac{5}{36}$	$\frac{5}{27}$	5.4
3	$\left.\begin{matrix} n = \frac{1}{9} \\ s = \frac{1}{16} \end{matrix}\right\}$	$= \frac{1}{9} - \frac{1}{16}$	$\frac{7}{144}$	$\frac{7}{108}$	15.4 +
4	$\left.\begin{matrix} n = \frac{1}{16} \\ s = \frac{1}{25} \end{matrix}\right\}$	$= \frac{1}{16} - \frac{1}{25}$	$\frac{9}{400}$	$\frac{9}{300}$	33.3 +
5	$\left.\begin{matrix} n = \frac{1}{25} \\ s = \frac{1}{36} \end{matrix}\right\}$	$= \frac{1}{25} - \frac{1}{36}$	$\frac{11}{900}$	$\frac{11}{675}$	61.4 —
6	$\left.\begin{matrix} n = \frac{1}{36} \\ s = \frac{1}{49} \end{matrix}\right\}$	$= \frac{1}{36} - \frac{1}{49}$	$\frac{13}{1764}$	$\frac{13}{1323}$	101.8 —
7	$\left.\begin{matrix} n = \frac{1}{49} \\ s = 1 \end{matrix}\right\}$	$= \frac{1}{49} - \frac{1}{64}$	$\frac{15}{3136}$	$\frac{15}{2352}$	156.8
8	$\left.\begin{matrix} n = 1 \\ s = \frac{1}{81} \end{matrix}\right\}$	$= \frac{1}{64} - \frac{1}{81}$	$\frac{17}{5184}$	$\frac{17}{3888}$	228.7 +
9	$\left.\begin{matrix} n = \frac{1}{81} \\ s = \frac{1}{100} \end{matrix}\right\}$	$= \frac{1}{81} - \frac{1}{100}$	$\frac{19}{8100}$	$\frac{19}{6075}$	319.7 +
10	$\left.\begin{matrix} n = \frac{1}{100} \\ s = \frac{1}{121} \end{matrix}\right\}$	$= \frac{1}{100} - \frac{1}{121}$	$\frac{21}{12100}$	$\frac{21}{9075}$	432.1 +
11	$\left.\begin{matrix} n = \frac{1}{121} \\ s = \frac{1}{144} \end{matrix}\right\}$	$= \frac{1}{121} - \frac{1}{144}$	$\frac{23}{17424}$	$\frac{23}{13068}$	568.2 +
12	$\left.\begin{matrix} n = \frac{1}{144} \\ s = \frac{1}{169} \end{matrix}\right\}$	$= \frac{1}{144} - \frac{1}{169}$	$\frac{25}{24336}$	$\frac{25}{18252}$	730.1 —
13	$\left.\begin{matrix} n = \frac{1}{169} \\ s = \frac{1}{196} \end{matrix}\right\}$	$= \frac{1}{169} - \frac{1}{196}$	$\frac{27}{33124}$	$\frac{27}{24843}$	
14	$\left.\begin{matrix} n = \frac{1}{196} \\ s = \frac{1}{225} \end{matrix}\right\}$	$= \frac{1}{196} - \frac{1}{225}$	$\frac{29}{44100}$	$\frac{29}{33075}$	

Distance in Lengths.	Comparative Influence of each Pole.	Relation of the Forces acting on the Compass.	General resultant, or actual Comp. influence. $F a^2 \frac{2x+1}{x^2 . \overline{x+1}^2}$	Ratio of resultant reduced.	
				To the relation R, being = 1.	To the relation of 1.0 inversely.
15	$n = \frac{1}{225}$ $s = \frac{1}{256}$	$= \frac{1}{225} - \frac{1}{256}$	$\frac{31}{57600}$	$\frac{31}{43200}$	
16	$n = \frac{1}{256}$ $s = \frac{1}{289}$	$= \frac{1}{256} - \frac{1}{289}$	$\frac{33}{73984}$	$\frac{33}{55488}$	
17	$n = \frac{1}{289}$ $s = \frac{1}{324}$	$= \frac{1}{289} - \frac{1}{324}$	$\frac{35}{93636}$	$\frac{35}{70227}$	
18	$n = \frac{1}{324}$ $s = \frac{1}{361}$	$= \frac{1}{324} - \frac{1}{361}$	$\frac{37}{116964}$	$\frac{37}{87723}$	
19	$n = \frac{1}{361}$ $s = \frac{1}{400}$	$= \frac{1}{361} - \frac{1}{400}$	$\frac{39}{144400}$	$\frac{39}{108300}$	
20	$n = \frac{1}{400}$ $s = \frac{1}{441}$	$= \frac{1}{400} - \frac{1}{441}$	$\frac{41}{176400}$	$\frac{41}{132300}$	
21	$n = \frac{1}{441}$ $s = \frac{1}{484}$	$= \frac{1}{441} - \frac{1}{484}$	$\frac{43}{213444}$	$\frac{43}{160083}$	
22	$n = \frac{1}{484}$ $s = \frac{1}{529}$	$= \frac{1}{484} - \frac{1}{529}$	$\frac{45}{256036}$	$\frac{45}{192027}$	
23	$n = \frac{1}{529}$ $s = \frac{1}{576}$	$= \frac{1}{529} - \frac{1}{576}$	$\frac{47}{304704}$	$\frac{47}{228528}$	
24	$n = \frac{1}{576}$ $s = \frac{1}{625}$	$= \frac{1}{576} - \frac{1}{625}$	$\frac{49}{360000}$	$\frac{49}{270000}$	
25	$n = \frac{1}{625}$ $s = \frac{1}{676}$	$= \frac{1}{625} - \frac{1}{676}$	$\frac{51}{422500}$	$\frac{51}{316875}$	
30	$n = \frac{1}{900}$ $s = \frac{1}{961}$	$= \frac{1}{900} - \frac{1}{961}$	$\frac{61}{864900}$	$\frac{61}{648675}$	
35	$n = \frac{1}{1225}$ $s = \frac{1}{1296}$	$= \frac{1}{1225} - \frac{1}{1296}$	$\frac{71}{1587600}$	$\frac{71}{1190700}$	
40	$n = \frac{1}{1600}$ $= \frac{1}{1681}$	$= \frac{1}{1600} - \frac{1}{1681}$	$\frac{81}{2689600}$	$\frac{81}{2017200}$	
45	$n = \frac{1}{2025}$ $s = \frac{1}{2116}$	$= \frac{1}{2025} - \frac{1}{2116}$	$\frac{91}{4284900}$	$\frac{91}{3213675}$	
50	$n = \frac{1}{2500}$ $s = \frac{1}{2601}$	$= \frac{1}{2500} - \frac{1}{2601}$	$\frac{101}{6502500}$	$\frac{101}{4876875}$	

Now these general resultants ought to be proportional to the *tangents* of the angles of deviation produced by the magnet at the respective distances. For, since the compass is acted upon simultaneously by forces * in two directions, that is, at right angles to each other, that of the earth, which may be considered as uniform, and always in the direction of the magnetic meridian, and that of the magnet varying as to intensity, but as to direction, being always at right angles to the other,—the measure of the variable force will be fairly given by the tangents of the angles of deviation produced in the needle. If, therefore, the law of the magnetic force as to distance be correct, that the action of either pole diminishes in proportion as the squares of the distance increase, then the tangents of the observed angles of deviation applied inversely to the general resultants, ought, on this hypothesis, to give the same product for all the different distances.

In order to verify these deductions, and to obtain a practical rule for determining the quantity of magnetic influence at all distances, however remote, a careful series of experiments were made with the two-feet bar magnet and one of Kater's five-inch compasses at the several distances of one to ten lengths.

The following Table contains the observed deviations, as produced by each pole of the bar, with the application of the tangent of the mean of the observed deviations to the reciprocal of the magnetic force acting on the compass at the respective distance, so as to produce the ratio, which was expected to be uniform, in the sixth column.

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* Though the forces are more in number than two, it is only necessary to consider the resultants of the forces in each direction.

Distance in lengths of the Magnet.	Deviation of the Compass Needle.		Tangent of the observed angles.	Magnetic force acting on the Compass.	Ratio of $\frac{\text{Tang. Dev.}}{\text{Mag. Force.}}$	Calculated tangents from a mean of col. 6 = 74000 × Magnetic force.	Resulting angles.	Differences.
	By each pole of the Magnet.	Mean.						
	° ′	° ′						° ′
1	$n =$ 34 2 $s =$ 34 30	34 16	68130	$\frac{1}{1}$	68130	74000	36·30	2 14 +
2	$n =$ 7 36 $s =$ 7 50	7 43	13550	$\frac{5}{27}$	73170	13703	7·48	0 5 +
3	$n =$ 2 44 $s =$ 2 42	2 43	4745	$\frac{7}{108}$	73208	4796	2·44	0 1 +
4	$n =$ 1 12 $s =$ 1 14	1 13	2124	$\frac{9}{300}$	71022	2220	1·16	0 3 +
5	$n =$ 0 44 $s =$ 0 40	0 42	1222	$\frac{11}{675}$	74984	1206	0·42	0 0
6	$n =$ 0 26 $s =$ 0 28	0 27	785	$\frac{3}{1323}$	79888	727	0·25	0 2 —
7	$n =$ 0 16 $s =$ 0 18	0 17	495	$\frac{15}{2352}$	77617	472	0·17	0 0
8	$n =$ 0 10 $s =$ 0 12	0 11	320	$\frac{17}{3888}$	73186	323	0·11	0 0
9	$n =$ 0 7 $s =$ 0 8	0 7½	219	$\frac{19}{6075}$	70022	231	0·8	0 0½ +
10	$n =$ 0 6 $s =$ 0 6	0 6	175	$\frac{21}{9075}$	75625	170	0·6	0 0

As the results in the 6th column appeared to differ too considerably to be at all satisfactory, the last three columns were added in order to determine the real nature and extent of the apparent discrepancies. Taking the mean of the ratios at 74000, in round numbers (which was found to be sufficiently near the truth), and applying that in a contrary way to the magnetic forces belonging to the different distances, the column of "calculated tangents" was obtained, the resulting angles of which, with only one or two exceptions, correspond with those obtained by experiment within the probable limits of error of observation *.

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* The compass employed being only graduated to 30′ of a degree, and without any vernier, it required some habit and attention to observe the angles within such small limits as those indicated by the column of differences.

These results, however, though sufficiently uniform and consistent for most practical purposes connected with the present investigations, were not so entirely satisfactory as to be received as conclusive. For the difference between the observed and calculated deviations at the first distance was by far too considerable to be ascribed to any error of observation or other accidental circumstance; since it was proved to be occasioned by some constantly acting cause, because of a similar discrepancy being always found to occur through an extensive series of experiments with many different magnets.

The discrepancies beyond the mere errors of observation were at length discovered to be mainly owing to the adoption of exact lengths of the magnets as integers of distance, instead of the length of the interval between what may be termed the *foci of attraction* in the north and south portions of the bar. For, although the principal energy of a well constructed and well magnetized bar is doubtless at the extremities, yet a considerable though rapidly diminishing power necessarily prevails *within* the extremities. The aggregate or resultant action, therefore, of the varying intensities of either half of the bar may be referred to a particular point or focus, which, through the application of the foregoing law of attraction, may be determined by approximation experimentally.

In order to determine the position of the foci of attraction in a three-feet bar-magnet (A) of very superior construction, I assumed, in the first instance, the *focal length* to be 2 feet 10 inches, considering the foci of attraction to lie an inch within the extremities of the bar. In this case the "Difference" in the first length, which in the preceding instance was 2° 14′, (Col. 9.) was now reduced to less than one-half. A focal length of 2 feet 9 inches was then tried, when a further reduction of error was observed. Lastly, I assumed the distance between the foci to be 2 feet 6 inches, and then the results were most strikingly consistent and satisfactory.

The following Tables exhibit the results of the first and last series of these experiments.

TABLE of the Results obtained when the exact length of the Bar was made the integer of distance.

No. of Lengths.	Mean Deviation.	Tangent.	Ratio of Tang. of Dev. / Magnetic Force.
1	36°.0′	72654	72654
2	8.19	14618	78937
3	2.56	5124	79056
4	1.23	2415	80500
5	0.46	1338	82104
6	0.28	815	82942

Table of Results with three feet Bar-magnet, when the distance between the foci was assumed to be 2 feet 3 inches.

No. of Focal Lengths.	Distance of nearest Pole to the Compass.		Deviation produced by each pole of the Magnet.	Mean.	Tangents.	Proportional.	Calculated Tangents from mean of 121600.	Resulting Angles.	Differences.
	Ft.	In.							
1	2	3*	n = 49.0 s = 52.36	50.48	122612	122612	121600	50°.34′	14′—
2	4	9	n = 12.0 s = 13.12	12.36	22353	120706	22518	12.41	5 +
3	7	3	n = 4.14 s = 4.48	4.31	7899	121870	7881	4.30+	1—
4	9	9	n = 1.48 s = 2.20	2.4	3609	120300	3648	2.5	1 +
5	12	3	n = 1.0 s = 1.16	1.8	1978	121377	1982	1.8	0
6	14	9	n = 0.29 s = 0.58	0.41	1193	121411	1195	0.41	0
7	17	3	n = 0.20 s = 0.34	0.27	785	123088	776	0.27	0

* The distance of the magnet from the compass at the first length, it will be observed, is three inches *less* than the assumed focal length. This apparent difference arises from the measurements of distance being taken as the interval between the centre of the compass and the nearest *extremity* of the magnet, instead of the nearest *focus* of attraction in the magnet lying three inches within the extremity.

In all my previous experiments, which amounted to from fifteen to twenty series, the tangent of the angle of deviation, at the distance of the first length of the bar, was always considerably *less* than the mean of the column of ratios, because of the assumed length between the foci of the magnet being *too great;* but in this last series, for the first time, the tangent belonging to one focal-length's distance is somewhat too great, which might seem to indicate that the assumed length was now *too small.* Where, however, all the ratios at other distances are found to coincide so very closely, the small difference in the first tangent must be referred to a peculiar cause, which, the greater discrepancies with shorter magnets eventually enabled me to detect.

In the foregoing investigations on the magnetic forces acting upon a compass, the attractive and repulsive actions of the same pole of the magnet on the different poles of the needle have been considered as a simple and not a compound action, because in most cases, excepting at short distances, they so combine as to produce almost exactly double the effect of either influence separately. This double effect, therefore, which for simplifying the investigation may generally be considered as the result of a single force, cannot, in the case of a short distance, and with a large compass-needle be so considered, without being the occasion of a very perceptible error. For although the north pole of a magnet, when placed at a considerable distance from a compass, in the direction of its east or west point, will attract the south pole of the compass, and repel the north pole with equal energy, the same magnet, if placed very near the compass, so as to produce a great deviation, will then have a difference of energy, as to its attractive and repulsive influences, because of the attracted pole of the needle being much *nearer* to the magnet than the pole which is repelled. Neither will the mean action of these two forces be the same as that of the force belonging to the distance as measured from the focus of attraction to the centre of the compass. As, for example :

Fig. 4.

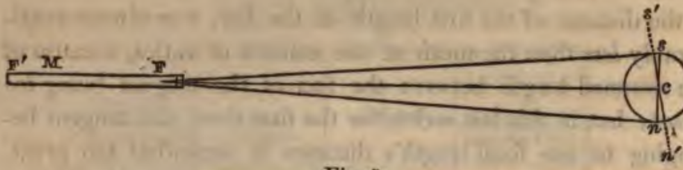
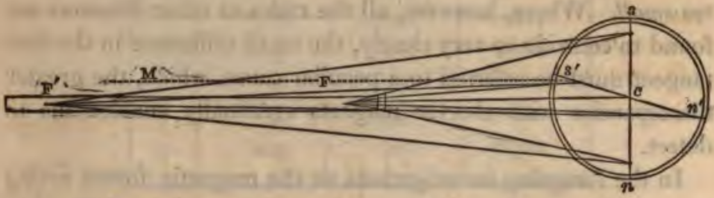


Fig. 5.



Let M, Fig. 4, be a magnet, of which F is the focal point, and C a compass at the distance of at least four focal lengths. Let *n s* be the position of the needle under the influence of terrestrial magnetism only, and *n′ s′* the position which it assumes under the action of the magnet. Then the action of the nearest pole F is represented by the sum of the squares of the distances F *s′* and F *n′* inversely, which does not, in this instance, materially differ from the sum of the squares of the distances F *s* and F *n* inversely.

But in the case represented in Fig. 5. the result is far otherwise. Here the bar M, placed at the distance of one focal length, occasions such a great deviation of the needle that a very considerable increase of action on the pole *s′* is gained, beyond the diminution of action sustained by the pole *n′*,—the increase of force being in the proportion in which the sum of the reciprocals of the squares of the distances F *s′* and F *n′*, expressed fractionally, exceeds the sum of the reciprocals of the squares of the mean distances F *s* and F *n* *.

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* The sum of these *direct* squares must in all cases be equal. For F *s n* being a triangle which is bisected in the point C by a line FC drawn to the apex, the sum of the squares of F *s* and F *n*, is equal to twice the squares of FC and C *s*. In the triangle F *s′ n′*, for the same reason, the sum of the squares F *s′* and F *n′*, is equal to twice the squares of FC and C *s′*. But the lines *s n* and *s′ n′*, representing the same needle revolving on a centre, are equal, and

But the nature and extent of this disturbing influence will be more evident, if we work out the case represented in Fig. 5, exhibiting the effect of the first distance of a 12-inch magnet (C) of 10 inches focal length, the powers of which, to the extent of six lengths, are exhibited in the following Table.

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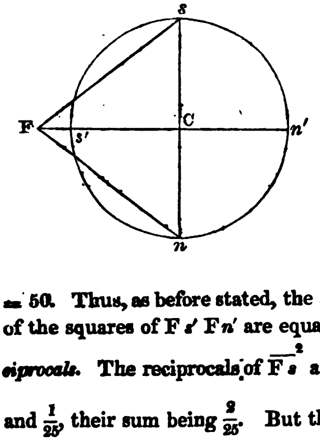
and FC common to both; hence twice the squares of each must be equal. Therefore the sum of the squares of F *s*, F *n*, and the sum of the squares of F *s′*, F *n′* being each equal to the same thing, must be equal to one another. Though, however, the sum of these several squares are equal—not so their reciprocals, as is clearly shown by working out the case referred to in Fig. 5.

Or, take a more simple case:



Let FC = 4, and C *s* = 3; then the figure F *s* C, being a right-angled triangle, $\overline{FC}^2 + \overline{Cs}^2 = \overline{Fs}^2$, which gives F *s* = 5. If the needle be now brought into the line FC, then F *s′* will be (4 − 3) = 1; F *n′* = (4 + 3) = 7; and FC, as before, = 4. Now, the sum of the squares F *s* and F *n* = 25 + 25 is 50; which is equal to $2\overline{FC}^2 + 2\overline{Cs}^2 = 32 + 18$ being also 50. In like manner, $\overline{Fs'}^2 + \overline{Fn'}^2 = 1^2 + 7^2 = 50$. Thus, as before stated, the sum of the squares of F *s* F *n′*, and the sum of the squares of F *s′* F *n′* are equal, each amounting to 50. But *not so the reciprocals*. The reciprocals of $\overline{Fs}^2$ and $\overline{Fn}^2$ $\left(= 25 \text{ and } 25, \text{ or } \frac{25}{1} \text{ and } \frac{25}{1}\right)$ are $\frac{1}{25}$ and $\frac{1}{25}$, their sum being $\frac{2}{25}$. But the reciprocals of $\overline{Fs'}^2$ and $\overline{Fn'}^2$ (= 1 and 49) are $\frac{1}{1}$ and $\frac{1}{}$ their sum being $\frac{1}{49} + \frac{49}{49} = \frac{50}{49}$. Hence, whilst the sums of the two sets of squares are equal, their reciprocals are found to be in the relation of $\frac{2}{25}$ to $\frac{50}{49}$, or as 1 to 12 nearly.

No. of Focal Lengths.	Distance of the nearest Pole from the Compass.		Deviation by Kater's Compass.					Tangent.	Ratio.
			Each pole of the Magnet.			Mean.			
	Feet.	Inches.		°	′	°	′		
1	0	9	$n =$ $s =$	56 62	26 20	59	23	168979	16898
2	1	7	$n =$ $s =$	13 14	48 36	14	12	25304	13664
3	2	5	$n =$ $s =$	5 5	2 10	5	6	8925	13770
4	3	3	$n =$ $s =$	2 2	14 30	2	22	4133	13777
5	4	1	$n =$ $s =$	1 1	15½ 19	1	17¼	2248	13794
6	4	11	$n =$ $s =$	0 0	47 46	0	46½	1352	13759
								Mean of 2 to 6, rejecting No. 1. =	13759
No. 1. repeated with a small compass, with a needle of $1\frac{6}{10}$ inches.									
1	0	9	$n =$ $s =$	55 56	0 30	55	45	146870	14687

Now, the position represented in Fig. 5. is that of No. 1, in the first line of this table, in which the magnet was placed at the distance of one focal length from the compass—not from the centre, but, in this instance, measured from the focal poles of the needle n, s.

Therefore the distances F s and F n are each = 1; and F′s and F′n the distances of the remote or counteracting pole are each = 2. But owing to the considerable length of the compass needle, and the great deviation which occurred on this occasion (namely 59° 23′), the actual distances of the two poles of the needle from the nearer focus F were $\frac{8}{10}$ and $\frac{2}{10}$, and from the remote focus were $1\frac{8}{10}$ and $2\frac{2}{10}$. Let us now see what relation of forces these distances afford in comparison with those belonging to the distance 1 and 2.

First, As to influence of the magnet on the compass, whilst the needle is in the meridional position s n.

The distances F s and F n being each = 1, their combined influence will be inversely as $1^2 + 1^2$ or $\frac{2}{1}$, which represents the whole force of the nearer focus of the bar which would act on the needle in the position *n s*.

Again, the distances of the remote focus F′*s* and F′*n* being each = 2, both their influences will be inversely as 2^2, that is 4 or $\frac{4}{1}$; the reciprocal of which, $\frac{1}{4}$, represents the respective forces. And $\frac{1}{4}+\frac{1}{4}=\frac{1}{2}$ represents the counteracting force operating on the needle in the position *n s*, at the distance 2. Hence $\frac{2}{1}-\frac{1}{2}=\frac{3}{2}$ represents the resultant influence of both foci, or of the whole bar in the given position.

Secondly, As to the actual influence exerted by the magnet in the deflected position of the needle *s′ n′*.

The distances F *s′* and F *n′* being $\frac{8}{10}$ and $\frac{12}{10}=\frac{4}{5}$ and $\frac{6}{5}$, their squares, representing the inverse power of their action, are $\frac{16}{25}$ and $\frac{36}{25}$. Hence the reciprocal representing the *attractive force* is $\frac{25}{16}$; and the reciprocal representing the *repulsive force* is $\frac{25}{36}$. Then $\frac{25}{16}+\frac{25}{36}=\frac{900}{576}+\frac{400}{576}=\frac{1300}{576}$ or $\frac{325}{144}$, which represents the whole influence of the *nearer focus* in the actual position, *s′ n′*, assumed by the needle *.

Again, the distance F′ *s′*, in the case before us, was found to be $1\frac{8}{10}$, and that of F′ *n′* = $2\frac{2}{10}$, or $\frac{18}{10}$ and $\frac{22}{10}=\frac{9}{5}$ and $\frac{11}{5}$, the squares of which are $\frac{81}{25}$ and $\frac{121}{25}$.

Hence the reciprocal representing the *attractive force* is $\frac{25}{81}$; and the reciprocal representing the *repulsive action* is $\frac{25}{121}$. Then $\frac{25}{81}+\frac{25}{121}=\frac{3025}{9801}+\frac{2025}{9801}=\frac{5050}{9801}$ = the whole influence of the *remote focus*, or *counteracting* forces, in the actual position, *s′ n′*, assumed by the needle.

Hence $\frac{325}{144}-\frac{5050}{9801}=\frac{3185325}{1411344}-\frac{727200}{1411344}=\frac{2458125}{1411344}$ represents the resultant influence of both foci in the deviated or actual position assumed by the needle, whilst the excess of this above the assumed force in the position $n\ s=\frac{3}{2}$, indicates the quantity of power gained by the magnet in consequence of the *length* of the compass needle.

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* It is here assumed that the attractive and repulsive forces are parallel to each other, which is not the case; hence the results obtained, though sufficiently near for our present object, can only be considered as approximations.

Reducing, now, the fraction $\frac{3}{2}$ to the same denominator as the above, we have $\frac{2117016}{1411344}$ as the resultant influence of the whole bar in the assumed position *n*, *s*. Therefore the entire or resultant force acting upon the needle, in the deflected position n', s', is to the force in the assumed position *n*, *s*, as $\frac{2458125}{1411344}$ to $\frac{2117016}{1411344}$, or as $\frac{7}{4}$ to $\frac{6}{4}$ nearly. If, then, we apply this proportion to the observed deviation of No. 1. (series in page 108), 59° 23′, the tangent of which is 168979, we have $\frac{7}{4} : \frac{6}{4} :: 168979 : 144839$, $= 55° 23′$, which, it is satisfactory to find, corresponds very nearly with the deviation observed when a very small compass was substituted for the large one; in that case, the angle formed by the needle, as near as could be observed, being 55° 45′.

Still, however, the deviation thus reduced is found to be considerably greater than that given by a mean proportional, namely, 54°. The cause of this difference is probably to be found in the peculiar direction, Fs', of the strongest force, which evidently is not strictly tangential to the meridional position of the needle; but must operate more favourably for overcoming the directive force of the earth, than if, acting in the direction s'M, it were precisely at right angles to the terrestrial magnetism.

Since now the calculated deviations of the three feet magnet (Table, p. 24), as obtained from the mean ratio 121600, are all, except the first, within the limits of the possible error of observation; and since the ratios obtained from experiments with the twelve-inch magnet (Table at p. 108) are all, with the exception of the first, uniform within the probable limits of error, —whilst the discrepancy at the first focal length has been sufficiently, I trust, accounted for,—the position of the foci in both these magnets may be considered as rightly determined *. For all practical purposes, therefore, connected with the proposed method of measuring the thickness of solid substances, a sufficient approximation, we find, may be readily obtained by experiment, for the position of the foci of attraction in any regularly magnetized bars whatever.

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* Though I have hitherto spoken of a fixed and determinate focal position representing the whole of the magnetic forces of either half a regularly magnetized bar, yet I am aware that that very focal position will be liable to a small variation at *very short* distances, in such cases being *nearer* the extremity than the calculated position. Nevertheless, at distances beyond the length of the magnet, no alteration in the position of the foci, I apprehend, will be discernible in practice.

Hence the law of the directive power of bar-magnets, at any distance, however remote, may be considered, I apprehend, as satisfactorily determined;

$$R \, . \, \frac{a^2 \, . \, \overline{a+1}^2}{2a+1} \, . \, \frac{2x+1}{x^2 \, . \, \overline{x+1}^2},$$

as before stated, representing the resultant action of all the forces in the magnet, at any distance, in measures of its focal length, or in fractional parts of such measures, on a compass situated in the line of the longer axis of the magnet, and bearing east or west from each other. And hence the directive force of all magnets, of proportional intensity of power, must be in the direct ratio of their focal lengths; so that if a bar of 1 foot focal length will produce a given sensible action on a compass (suppose of 5′), at the distance of 12 feet,—then a bar of 2 feet focal length, proportionally strong, will produce the same deviation at 24 feet, a bar of 3 feet focal length at 36 feet, or one of 6 feet interval between the foci of attraction, will produce a similar sensible deviation at the distance of 72 feet.

3. The foregoing investigations enable us satisfactorily to determine *The extent to which this method of ascertaining distances may be carried.*

As the directive power of different magnets, similar as to proportions and quality, is very nearly in the relation of their lengths § 1. (2) *b*; and as the accuracy of different well-constructed compasses, as to their capability of indicating very minute quantities of magnetic influence, may be considered to be in the relation of the lengths of their needles,—it is evident that there can be no assignable limit to the application of the method of ascertaining distances now proposed. For, by increasing the length or number of the magnets, and improving the quality or increasing the dimensions of the compass-needles, still further and more extensive applications of the principle will be obtained.

A few of the results, however, with ordinary compasses and moderate-sized bars, may be useful, as illustrative of the facility of employing the magnetic influence practically for the purpose proposed.

With a single straight bar-magnet, and tempered only at the ends, and a pocket-compass of only 1¾ inches diameter, distances of four times the length of the bar may be determined to within 1-10th of the whole, and shorter distances to a more considerable degree of accuracy.

With the same single bar, and a Kater's compass of 5 inches in diameter, distances of about six times the length of the magnet may be determined within a very moderate limit of error, and an extent of even ten lengths of the bar may be measured to, perhaps, a tenth of the whole.

Two similar magnets placed parrallel to each other, a few inches asunder, will indicate, within the same limits of error, distances of nearly a length and a half more remote with the pocket-compass; or with a Kater's compass, distances from two to three lengths greater, when the deviations are very small.

Four bar-magnets of a similar kind will produce, on the Kater's compass, about the same deviation at 16 *focal* lengths, as a single bar produces at ten focal lengths—thus affording, with three feet magnets, a tolerably correct measure of the thickness of any solid intervening substance, of about 40 feet. But the same magnets will produce a sensible effect on the compass (namely, a sum of deviations amounting to 2′) at the distance of no less than 33 focal lengths, or 82 feet!

These results, however, which are taken from the observation of the effects of bar-magnets on a compass acted upon by the whole force of terrestrial magnetism, will be greatly modified, and vastly extended, when the directive force of the earth on the compass is partially neutralized, by the proper arrangement of one or two small magnets in juxtaposition. For, extending the influence, therefore, to the greatest possible distance with a given apparatus, such an arrangement will of course be used.

But I proceed to give the particulars of a few experiments by which the extent of influence expected to be produced was amply verified, and, by which an increasing influence, under improved arrangements, was progressively produced.

The first experiment in which any very great distance was attempted, was made with four straight bar-magnets of two feet in length each (two of them, however, were very weak, and added little to the result), with a view of endeavouring to convey the magnetic influence through my own house. The compass, in this instance, was placed outside of the northern, or main back-wall, a little *above* the level of the dining-room floor, and the magnets were placed on the south side of the southern or main front-wall, a little *below* the level of the dining-room floor,—so that the magnetic influence, if it should affect the compass, might pass, not only through the intermediate space of 25 feet 6 inches, but, at the same time, through the two principal walls of the house, and obliquely through all the joists of the floor, with other interposing substances. In order to render the compass as susceptible as possible, the directive force of the earth's magnetism was very greatly diminished by a neutralizing magnet placed near it; whilst, in each experiment, the magnets on the opposite front of the house were simultaneously turned, with their analogous poles the same way, so as to influence the compass by the sum of their action. Three sets of experiments were now made, when the results gave an amount of deviations of 0° 20′, 0° 15′, and 0° 15′. Whilst the compass, with its neutralizing and directing bar were undisturbed, the magnets were brought round into the back garden, and placed at the distance of 25 feet 6 inches from the compass in the opposite direction, in which there was no interposing object;—the deviations now produced by changing the poles of the magnets simultaneously, were, in this position, very nearly the same as before, being, on each of three successive experiments, 0° 15′. This was decided evidence of the great extent at which the directive power of the magnet is capable of influencing the compass, and indicated that the measure of distances, even within a quarter of a degree of deviation, or less, might be accomplished with considerable precision.

On subsequent trials, however, a visible action on the compass was produced by the influence of a pair of two feet magnets only, at much greater distances.

The compass, in the next trial, was placed upon the library chimney-piece, and the needle so far neutralized, that its vibrations were reduced from 13 to 8 per minute. A pair of two feet magnets were then taken to the farther extremity of a bed-room on the same floor, separated from the compass by the width of two rooms and a lobby, with two brick-walls, book-case containing books, &c.; and there, in a parallel position, and about 10 inches asunder, presented to the compass. Their action, though at the distance of 31 feet 8 inches, was quite evident and measurable. The amount of the influence of their poles, in a mean of six sets of experiments, was 0°.23′⅕ *.

Having at this stage of my experiments obtained a beautiful pair of magnets (A), before referred to, from Sheffield, constructed under the kind superintendence of Mr Abraham, I was enabled to accomplish every thing which my calculations had anticipated. These magnets measure exactly 3 feet in length, 2¼th inches in breadth, and ¼th of an inch in thickness. They are made out of the best cast-steel, and tempered throughout,—the ends, after the whole was hardened, having been reduced to a gold colour, and the rest of the bars to a blue. The following table exhibits their powers in combination, as they lie in their case, 4½ inches asunder, in that reduced state of magnetism produced by being repeatedly placed in unfavourable contact; when magnetised, however, to saturation, their action on the compass is still more considerable.

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* In very delicate experiments, it is found to be advantageous to the action of the compass gently to tap the glass with the point of the finger; without this precaution, the friction of the needle on its point may be productive of considerable discrepancies.

No. of Focal Lengths.	Distance.		Force of the Magnets, R = 1.	Tangent from mean of 210000.	Corresponding deviations by either Pole.			Sum of the deviations of both Poles.		
	Ft.	In.			°	′	″	°	′	″
1	2	3	$\frac{1}{1}$	210000	64	32	12	129	4	24
2	4	9	$\frac{5}{27}$	38889	21	15	0	42	30	0
3	7	3	$\frac{7}{108}$	13611	7	45	4	15	30	8
4	9	9	$\frac{9}{300}$	6300	3	36	18	7	12	36
5	12	3	$\frac{11}{675}$	3422	1	57	34	3	55	8
6	14	9	$\frac{13}{1323}$	2064	1	10	56	2	21	52
7	17	3	$\frac{15}{2352}$	1339	0	46	2	1	32	4
8	19	9	$\frac{17}{3888}$	918	0	31	33	1	3	6
9	22	3	$\frac{19}{6075}$	657	0	22	35	0	45	10
10	24	9	$\frac{21}{9075}$	486	0	16	42	0	33	24
11	27	3	$\frac{23}{13068}$	370	0	12	44	0	25	28
12	29	9	$\frac{25}{18252}$	287	0	9	52	0	19	44
13	32	3	$\frac{27}{24843}$	229	0	7	52	0	15	44
14	34	9	$\frac{29}{33075}$	184	0	6	18	0	12	36
15	37	3	$\frac{31}{43200}$	151	0	5	12	0	10	24
16	39	9	$\frac{33}{55488}$	125	0	4	18	0	8	36
17	42	3	$\frac{35}{70277}$	105	0	3	37	0	7	14
18	44	9	$\frac{37}{87723}$	89	0	3	4	0	6	8
19	47	3	$\frac{39}{108300}$	76	0	2	37	0	5	14
20	49	9	$\frac{41}{132300}$	65	0	2	14	0	4	28
21	52	3	$\frac{43}{160083}$	56	0	1	56	0	3	52
22	54	9	$\frac{45}{192027}$	49	0	1	42	0	3	24
23	57	3	$\frac{47}{228528}$	43	0	1	29	0	2	58
24	59	9	$\frac{49}{270000}$	38	0	1	18	0	2	36
25	62	3	$\frac{51}{316875}$	34	0	1	10	0	2	20
30	74	9	$\frac{61}{648675}$	19.8	0	0	41	0	1	22
35	87	3	$\frac{71}{1190700}$	12.5	0	0	26	0	0	52
40	99	9	$\frac{81}{2017200}$	8.4	0	0	17	0	0	34
45	112	3	$\frac{91}{3813675}$	6.0	0	0	12	0	0	24
50	124	9	$\frac{101}{4876875}$	4.4	0	0	9	0	0	18

With these magnets the following experiments, shewing their influence through solid substances, were made.

Near the engine-house of the Liverpool and Manchester Railway, I found a very favourable position for trying the effect of the magnetic influence on the compass, through a solid mass of freestone rock *in situ*. In the place referred to, the rock is excavated in two parallel positions to a considerable depth into the solid. Between the two is a solid septum 31 feet 5 inches in thickness. The magnets being placed on the one side, and the compass on the other, the position of the needle, (which happened to be so nearly parallel to the wall as to require no directing magnet), was observed, and then the opposite poles of the magnet were presented, which occasioned a deviation of about $\frac{1}{3}$ of a degree. The experiment was repeated several times with a very uniform result, affording a mean deviation, being the sum of the action of the north and south poles, of about 20′ of a degree, whilst the calculated deviation corresponding with the total distance of 32 feet 1 inch, viz. 31 feet 5 inches of rock, and 8 inches, the distance of the centre of the compass from the rock, was 19′.

On a subsequent occasion, I attempted to transmit the magnetic influence through about 60 feet of rock near the same place as that of the above experiment; but the quantity of "*live-iron* [*]" on the spot, consisting of locomotive engines perpetually passing to and fro, produced such frequent changes on the compass as to defeat my object in this experiment. In consequence of this disappointment, I sought in my own premises for a situation suited for my purpose, which the sloping nature of the ground in front of the house readily presented. Placing the magnets in the front garden, and the compass within the house in a cellar kitchen, the magnetic influence had to pass obliquely through an extent of soil, stones, and brick-walls, of no less than 61 feet, in order to act on the compass. But even at this distance its influence was very perceptible. The mean of six experiments indicated a deviation under the action of the opposite poles of 0°.3′, whilst the calculated action, at $24\frac{1}{2}$ focal lengths, or 61 feet 6 inches, was found to be 0°.$2\frac{1}{2}$′! The influence of terrestrial magnetism was now partially neutralized by the juxtaposition of a small magnetic bar, when a series of five experiments gave a mean of 0°.$4\frac{3}{5}$′. Another series of six experiments, with an alteration in the position of the neutralizing magnet, gave a mean deviation of 0°.$5\frac{1}{3}$′. The reality of the influence, therefore, under these circumstance, through a distance of 61 feet of earth, stones, and bricks, was unquestionable.

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[*] The proximity of stationary iron has no injurious influence on experiments of this kind whatever; but iron in motion, if sufficiently near to act upon the compass, must necessarily produce an unequal influence.

Results strikingly satisfactory were also obtained, on subsequent trials, in the south of Ireland, with the two feet magnets, (B) through solid masses of limestone-rock. The following abstract shews the surprising coincidence of the " observed" and " calculated results."

Observed Results.				Calculated Results.				Difference of Observed and Calculated results.
Measured Distance.		Number of Observations.	Mean Deviation of both Poles.	Number of Focal Lengths.	Equivalent Distances.		Expected Deviation.	
Feet.	In.		′ ″		Feet.	In.	′ ″	′ ″
24	9	12	9.10	15	25	0	9.32	0.22
40	5	8	3.10	24	40	0	2.23	0.47
43	9	10	1.36	26	43	4	1.54	0.18

III. THE PRACTICAL APPLICATION OF THE MAGNETIC INFLUENCE IN ENGINEERING AND MINING OPERATIONS, IN ORDER TO THE DETERMINATION OF THE THICKNESS OF SOLID SUBSTANCES NOT OTHERWISE MEASURABLE.

The foregoing investigations, defective as they are in attainable accuracy, are nevertheless amply sufficient for *the practical application of the present method of determining the thickness of solid substances, not otherwise measurable.*

The cases in which this mode of measuring distances seems to be most particularly suitable, and where, it appears to me, its application might often be of considerable importance, chiefly occur in mining.

In tunnelling, for instance, through rock, and working from different shafts, this method of measuring the intermediate distance would often relieve the engineer from much anxiety, and the miner from much personal risk, when the opposite workings come near together. For although practical men, who have been well experienced in mining operations, in that particular kind of rock, may make a tolerable guess of the distance between the two "head-ways" previous to their breaking through, yet, it is well known, that very experienced engineers have often estimated the interposed rock at some yards in thickness, when, in reality, it has not been more than as many feet. Blasting, under such a false supposition, it may be well imagined, might be attended with the most fatal consequences.

A very serious accident, that had well nigh proved fatal to two very deserving men, happened in the working of the railway tunnel under the town of Liverpool, which will very well illustrate the present case. This tunnel, which is 2250 yards in length, was excavated not only from the extremities, but also from six or seven intermediate shafts. The person entrusted with the superintendence of the working department, being a practical man, was aware of their near approach to a junction, and arranged a signal with the opposite party previous to their making an intended blast. But the workman charged with this duty, it would seem, did not believe that the junction was so near, for whilst the confiding "overlooker," and a companion on the opposite side, were listening to the sound of the picks, the thoughtless miner, without giving the signal, applied the match, when the force of the discharge, instead of re-acting, exploded forward about the defenceless heads of the unfortunate individuals in the contiguous head-way. Most providentially, they escaped with their lives; but they were severely wounded, their faces permanently blackened by the penetration of unexploded gunpowder, and both of them suffered the loss of an eye!

In this instance, had the distance been accurately determined, which it might have been by the magnetic influence to within a quarter of an inch, they never would have attempted a blast under such perilous circumstances, or at least no man would have been so reckless of the safety of his fellow-workmen, as to have made the discharge without a sufficient warning.

Another important application of the proposed method of ascertaining distances, is in the working of mines. When approaching an old working from a different shaft, or the verge of an adjoining mine—perhaps a different property,—many resons might exist, such as an accumulation of water, "fire-damp," &c. why the separating wall should not be perforated; whilst, on the other hand, the value of the ore or bed of coal might be such as to render it desirable to approach as near as consistent with safety. No means, however, heretofore known, would enable the miner to ascertain with accuracy the thickness of the interposing wall; whereas, by the proposed method, whenever the two sides of the wall were attainable, the thickness, within given limits, might easily be determined. And the cases are by no means unfrequent, in which a magnet, under proper precautions, might be safely carried into an old working containing fire-damp or water, though the breaking through into that working might be attended with most mischievous or even dangerous consequences.

In mining beneath the sea also, or under the bed of a river, the same principle (substituting a dipping-needle for the compass) may be easily applied, so as to determine the thickness of the stratum of earth or rock forming the roof of the mine, and indicative of the sufficiency of the protection against overwhelming waters.

Many supposable cases might occur in engineering, or in ordinary architectural works, in which the principle before us would have important application. Suppose, for instance, that it is proposed to build a spire upon the tower of a church, or any other heavy superstructure upon a given wall, but it is not known, from the want of embrasures or windows in some of the sides of the tower, whether each portion of the basement be sufficiently strong to sustain the additional weight. Instead of perforating the wall, where no ordinary means exist of ascertaining its thickness, which might be laborious, and perhaps a blemish to the interior, the magnet would readily give the thickness to the greatest desirable accuracy. In like manner, in engineering, the thickness of walls, or beds of rock, not otherwise determinable but by actual perforation, might, in most supposable cases, be measured by means of the magnetic deviations.

The verifying of masonry or brickwork executed by contract, and not sufficiently inspected, is another application of this principle which might very often be useful, by enabling us, without injury to the structure or finishing, however delicate, to determine the thickness of almost every portion of the blank walls with almost perfect accuracy.

Mr George Stephenson, the talented engineer of the Liverpool and Manchester railway, suggested another application, which, he conceived, might frequently be exceedingly useful in the working of coal. In many cases it is the practice of the colliers to run two parallel drifts through a great extent of coal, leaving between them a wall, from 10 to 20 feet thick, for the support of the roof, but which it is desirable should not be thicker than necessary, to prevent the waste of labour and coal. In this arrangement, which is often adopted when the roof is loose, so as not easily to be supported, it is a matter of great practical difficulty to prevent an undue encroachment upon the limited thickness, that the safety of the roof be not endangered. In such a case, the apparatus for the deviation might be applied with much advantage, and the thickness verified, as occasion might require.

An engineer in extensive practice in Scotland, who recently visited Liverpool, and happened to be present at the "railway area" when I was trying the magnetic influence through a great body of solid rock, expressed his exceeding delight in what he witnessed, and mentioned several instances, in his experience, in which the present process would have been of incalculable advantage. Two of the cases it may be satisfactory to mention.

In an extensive colliery, with the general management and working of which he was entrusted, a horizontal drift, designed for carrying the water off the mine, by conveying it to the well of the pumping-engine, was stopped up by the falling of a portion of the roof. This circumstance, which completely put a stop to the chief operations of the mine, occasioned them great anxiety and perplexity, from their utter inability to determine *the extent* of the fallen rock. Not knowing how small that extent might be, they did not dare to clear any part away *below* the stoppage, lest the water should burst through and overwhelm the workmen; and to attempt to clear it from the upper part, where the water had risen within a yard of the roof, was a matter of much difficulty. Nor did the examination of the upper part, by the engineer himself lying flat in a float, and pushing himself forward by his hands against the roof, give him any satisfaction, further than this, that the portion of fallen rock being solid, must be shaken with a blast, as affording the only means of clearing away the obstruction. This hazardous experiment, after much delay and anxiety, was at length undertaken and cleverly accomplished; and, fortunately, the obstruction proving to be very partial, the water made its way through the disrupted materials. But it was a mere chance, as they knew nothing of the thickness of the fallen masses. Had the thickness been many times as great, they never could have ascertained how long it would have been safe to have worked below the obstruction, nor when a blast at the upper part would have been the most advantageous. This, however, the magnetic deviation would have enabled them readily to determine, and then they would have proceeded at once to the most proper measures, and been relieved from the greatest embarrassment.

Another case which he mentioned was more important. In the sinking of a coal-mine near the Frith of Forth, it was required, on one occasion, to run two parallel drifts or tunnels beneath this arm of the sea. It was essential, in this work, that the two drifts should be near each other and parallel, but, for the success of the future operations, they must not perforate the separating wall. When, however, the tunnels had proceeded a considerable distance, the workmen, misled by some mistake or neglect on the part of the superintending miner, broke through the septum, and the whole work, which had been conducted so far at a great expense, was rendered entirely useless. Here, then, was an instance in which magnets could have been employed with the greatest facility, and *must* have been effectual in preventing the accident which ruined the work.

Though many more cases may easily be imagined in which the process before us might be of important application, enough, I trust, has been said to prove that it is not a mere matter of philosophical curiosity, but is capable of becoming of extensive and practical utility.

It still remains for me to describe the requisite apparatus, with the preparation of the magnets, and to offer a few suggestions for carrying into the practice of mining, &c. the principles developed by the preceding investigations.

The apparatus necessary for the practical objects in view are chiefly a pair of bar magnets, tempered throughout, of 3 feet or upwards in length; another pair, say of 12 inches, corresponding in quality, temper, and proportions; a compass (the one employed for mining operations will do), with two or three other articles, of simple structure, hereafter to be described, which may be made by any ordinary artizans. The preparation and determination of the powers of the magnets will be the first step in the business.

The bars having been magnetized to saturation, will require to have their directive powers and foci of attraction determined. Previous to this, it will be prudent to place them upon one another for a few moments, with similar poles contiguous, not in contact, but separated by a sheet of paper, or other thin substance, in order to disperse or neutralize any excess of magnetic energy which they might not be able permanently to retain. Without this or some similar precaution, the intensity of the magnetic energy of the bars will be liable to diminution in the course of the experiments, so as to prevent satisfactory or indeed useful results. The position of the foci of attraction may then be easily determined. And in accomplishing this, for ordinary practice, it will only be necessary to try the deviation at the first three or four lengths. Assuming the focal position, as in the experiments heretofore made, at $\frac{1}{12}$th of the whole length from each extremity, we shall generally, in properly magnetized and well-proportioned bars, be very near the truth.

But, by the way of example, let the position of the foci of attraction of a three-feet bar, or of a pair of such magnets in combination, be required. The assumed position of the foci of attraction, at $\frac{1}{12}$th the length within each extremity, will give 2 feet 6 inches for the focal length, and 3 inches for the distance of each focus from the extremity. The first focal length, because of the measurements being from the centre of the compass to nearest end of the magnet, instead of the nearest focus, will be 2 feet 6 inches — 3 inches = 2 feet 3 inches. The second $2 . 3 + 2 . 6 = 4$ feet 9 inches; the third $4 . 9 + 2 . 6 = 7$ feet 3 inches, &c. Find now the mean deviations, by observation, of the two poles of the magnet at these several distances, and multiply the tangent of the deviations by the reciprocals of the relative forces before determined (Tables at pp. 99 and 115); then, if the assumed position of the foci be correct, the numbers thus obtained will correspond very nearly with the tangent of the angle of the greatest deviation, as well as with each other. If, however, the tangent of the greatest deviation be *less* than these remoter products, then the assumed position of the foci will be too near the extremity of the magnet; but if the nearest tangent be *very much* greater than the rest (we speak here of a 3-feet magnet), then the position assumed will be too far from the pole. As, from principles already explained, the first tangent ought to be somewhat greater than the products of the other tangents*, it will be desirable, when a tolerably close approximation has been obtained, to verify the result by trying the deviations at greater distances.

What has been now suggested for the determination of the foci of attraction, is equally applicable to two or more bars in fixed combination, or at a given distance, as to single bars. A convenient arrangement, represented in Fig. 6, designed for the safe keeping of the magnets and their employment in practical operations, will be well adapted for giving fixedness of position during the determination of their powers. Here, the magnets A 1, A 2, are laid in parallel grooves in a case about four inches apart. C, C, are the neutralizing conductors of soft iron, which, uniting the opposite poles of the magnets when not in use, serve to preserve their energy uninjured. *a a* are two pieces of wood fitted into grooves in the centre of the case, of the same size as the conductors. Such is the arrangement when the bars are required to be neutralized, and always when unemployed. When required in the mine, it is only necessary to change the position of one of the bars end for end, so that the two similar poles may lie contiguously, at the same time removing the conductors into the grooves at *a a*, and substituting in their stead the pieces of wood; by this means the magnets are retained firmly in their places, and the whole apparatus kept together.

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* On account of this difference being so great, when short magnets, or very long compass needles, are employed, the *first* length must be rejected from the results, and the approximation for the focal position be determined by the observations at the more remote distances.

Whilst thus retained in their case, the powers and the focal position of both bars together are determined, so that their condition in practical operations is exactly the same. A table of their powers (after the manner of that given in page 115) may then be calculated, and the bars forthwith employed as occasion may require.

But I proceed with some suggestions for the practical application of the foregoing principles and investigations in mining.

In all cases where both the level and direction of the workings or mines are known to be the same, or in which the difference can be satisfactorily ascertained, the application of the proposed process for determining the thickness of interposing substances, will be sufficiently plain from what has already been stated; for, in such cases, a single observation of the deviation produced by the action of one or more magnets will be sufficient to show the distance betwixt the magnet and the compass. When the direction of the separating wall lies *nearly* north and south, so that the compass does not require any directing magnet, the table of deviations already calculated for the magnets in use will enable the miner generally to judge with sufficient accuracy of the distance, without the trouble of bringing the magnets round to make the corresponding experiment*. And even where a directing or neutralizing magnet is employed, it would not be difficult to come at very satisfactory results by the employment of the smaller set of proportional magnets (proportional as to dimensions and directive power), so as generally to render it unnecessary to remove the bars from one mine to the other. For if the two pairs of bars were constructed at the same time, of the same quality of metal, and of similar temper, there would be little difficulty of giving them a directive power, which, at considerable distances, in reference to their focal lengths, would be very nearly, or almost exactly, the same; and, indeed, were there some little difference in the magnetic intensity of the two sets of bars, it would be no difficult matter to ascertain the relative force of each, and to obtain sufficiently accurate results, by calculation, accordingly. The appropriation of the results obtained from the smaller set of magnets for the determination of the distance of the larger magnets from the compass, when the same deviation was produced, would be most simple—the measure of the distance given by the smaller magnets being to the real distance through the rock, as the focal length of the smaller is to that of the larger magnets. If, indeed, the smaller magnets were a foot, and the larger three feet in length, the distances at which they would produce equal deviations, supposing their magnetic energy proportional, would be just as one to three.

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* Should the proposed plan of measuring the thickness of rock, &c. be brought into general practice, it would be useful and important to have tables constructed of the form of that in page 35, shewing the ratio of deviations for magnets of different degrees of energy. A series of tables, for instance, might be calculated for every degree, or for every two degrees, of deviation at *one* focal length, from 55 or 60 to 75 or 80 degrees.

Another method might be adopted for reducing the observed deviation under a neutralizing or directing needle to the ratio in the table of deviations, which is by finding the number of oscillations of the compass needle in a given time, after its adjustment in the mine, and by comparison with its ordinary vibrations under terrestrial magnetism only, calculating the relative magnetic forces contending against the action of the magnet *. But this method being troublesome, the former is to be preferred for ordinary practice.

As in many cases, however, the comparative levels or directions of the two workings might not be satisfactorily known, a single experiment, though it give the distance between the compass and the magnet, will not be sufficient to determine the nearest approximation of the two contiguous mines; but the nearest approximation, I apprehend, may be very well determined, and at the same time,—a matter of no mean importance,—the comparison of level and direction, as well as the distance of the workings. It may be useful, however, for the sake of perspi-

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* Here the intensities of the magnetic forces, under different circumstances, will be proportional to the squares of the numbers of horizontal oscillations in equal times.

cuity and practice, to consider this application of the principle under three separate cases.

Case I.—*Where the level of the mines is known to be the same, but the direction is doubtful.*

In this case it is only necessary to apply the magnets in one of the mines in a given position, and then at the same horizontal level of the other, (say on the floor of the mine, or at any particular distance above), to try the deviations of the compass at moderate intervals, from side to side, directly across the headway, when, from the observation of the *greatest* deviation, the point nearest to the magnets may be ascertained. One precaution, however, will be necessary. If the direction of the compass-needle be not exactly at right angles to the position of the magnet, then the greatest deviation will not occur at the nearest point but a little *beyond*, towards the place where the action of the magnet is most perfectly tangential *. Hence, it will be generally advisable, not only to adjust the needle of the compass, by a directing magnet, as near as may be to a right-angular position with the magnet (as shown by the known magnetic position of each mine), but also to repeat each experiment with both poles of the magnet alternately presented to the compass, so as to obtain the difference of deviations of the two poles at each position of the compass. In order to conduct this experiment with the greater facility and uniformity of effect, the simple apparatus represented in Fig. 7, Plate V. of preceding volume, formed out of a piece of board, may be useful.

C is the compass, and D a *directing* magnet sliding in a groove, which is so adjusted as to its distance from the compass, that the needle may assume a position parallel to the straight edge of the board *a a*, when that edge is placed parallel to the interposed substance, the thickness of which is to be ascertained. The adjusting magnet is then fixed in its place by the thumb-screw *s*, so as to secure a constant influence, whilst the parallelism of *a a* with the wall of the mine is preserved; and, in order to facilitate and secure the preservation of that parallelism, a deal plank may be placed directly across the mine against the extremity of the working, and supported at the required level, which would not only serve as a table for the *compass apparatus*, but having nailed upon it a straight batten near and parallel to the edge, it would enable the experimenter to bear the compass from side to side of an irregular working, and to preserve in every part of the distance the parallelism of the straight edge *a a*. Fig. 8. Plate I. illustrates this arrangement.

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* As this circumstance might in some cases occasion a considerable error, if only the deviation by one pole were taken, it would be useful to have the means of ascertaining the extent of error capable of being thus produced. This would be given by a table of the powers of the magnets, calculated from the *sines* of deviation instead of the *tangents,* which would shew the distance of the magnet from the compass, as placed at right angles to the needle in its deviated position. At great distances, however, no material mistake can arise from the neglect of this source of error,—the powers of the sines and tangents being so nearly the same in deviations under 5 or 6 degrees as to be within the ordinary limits of error of observation.

Suppose T to be the working of a mine, and T′ the approximating working, and it is required to ascertain the thickness of the intermediate mass of rock R. The case of magnets M being placed against the middle of the wall, in one of the *headways*, the plank P is placed at the same horizontal level on the opposite side, when the compass apparatus, guided by the batten *e e*, being carried from side to side, will shew the greatest deviation in the position *a*. Let the magnets be now turned with their opposite pole towards the compass, (the time for doing which is to be indicated by signal, as about to be suggested), and the deviations tried a second time across the mine. If the greatest deviation be in the same position as before, *that* may be assumed as the nearest point; if not, the mean of the two positions must be taken.

It is not necessary to describe the experiments more minutely, nor to enter into the management of the apparatus for all particular conditions; for to do so would swell this paper to an inconvenient length, whilst the details would be easily anticipated by a very little practice. It may be proper, however, to mention, that where the deviations on the removal of the compass continue to increase to the very extremity of the board, or to the side of the headway, it will be necessary to cause the magnet to be removed in the opposite direction, so that the maximum deviation may be satisfactorily obtained.

Case II.—*Where the direction may be certain, but the level doubtful.*

Here the magnet and compass being placed on the known line of direction, the compass is moved progressively upward, from the floor to the roof of the mine, to determine the greatest deviation, which, it is evident, will be on the same horizontal level as that of the magnet.

A result, perhaps more satisfactory, might be obtained by the use of a needle, suspended like a dipping needle, and traversing in a vertical plane. Such a needle being brought into a vertical position by a small directing magnet, would shew by its deviations the position of corresponding level.

Case III.—*Where both level and direction may be uncertain.*

In this instance, the magnet being fixed, and the plank placed on the floor of the mine, the compass apparatus is to be carried from side to side, till the place of the greatest deviation is ascertained, or the middle between the two positions of greatest deviation produced by the different poles. This will give the directional position. The vertical needle being now tried throughout a vertical line drawn through the directional point, will shew by its greatest deviation the horizontal level of the magnet, which, being at the same time in the directional line, will indicate the nearest attainable point between the magnet and compass. The vertical needle, in this and similar cases, may be guided in its transit by a board, with a straight edge, or pillar of wood, placed vertically, so as to preserve its parallelism of position at every altitude.

It may be proper here to call to mind, that the chief object of the processes now described, is the determination of the thickness of solid substances not otherwise measurable, and not primarily at least, the determination of direction and levels; for the application of the observation of deviations to these latter purposes is only expected to afford an approximation, in some cases, indeed, a very important approximation, to the truth. Where the distance to be measured is small, say only 8 or 10 feet, or in any case in which the deviations amount to 3 or 4 degrees,—the line of direction may be very accurately determined by the middle point between the maximum deviations of the opposite poles of the magnets, and especially if each maximum be itself determined by the mean of *equal deviations* on both sides of the maximum; in this case I have frequently determined, at moderate distances, the very point on the cross plank, from whence a line drawn at right angles will pass through the magnets, consequently the nearest possible distance between the magnets and compass. Where the deviations, however, are very small, as it will then require a considerable change in the position of the compass, laterally, to produce any sensible change in the deviation,—the line of direction, or the level, will be proportionally less certain, though the nearest distance may be sufficiently ascertained.

As the person who conducts any experiments on the foregoing principles will have to direct the application of the magnets, with any changes that may be requisite as to their poles or position, whilst the experiment is in progress, an arrangement of directions by *signal* will be necessary; and this may be very well accomplished by the blows of a hammer or mall.

The distance at which any sound can be heard through solid substances, will depend both on the nature of the concussion, and the quality of the vibrating mass. Through earth, gravel, or other loose materials, the transmission of sound is very limited and uncertain; but through solid uniform rock, directions may be communicated by signals to very considerable distances. In solid and uniform sandstone, the pick, I understand, may be very well heard through a distance of fifteen or sixteen yards, whilst the vibration produced by a blast may be perceived at very great distances beyond. The working of coal by the pick may be generally heard sixty or eighty feet, I am told; though no rational determination of the thickness can be made from the nature or intensity of the sound,—for a change in the structure of the bed, or the interposition of an increased quantity of moisture, may produce as great an alteration in the sound as might be expected if the distance were doubled! The blow of a mall upon an iron wedge set in the rock, may be heard much farther than the stroke of a pick, or the blow of the heaviest hammer on the face of the rock,—and this will form a convenient arrangement for the purpose of signals.

By the use of two elements,—number of blows and an extra interval of time in different parts of the series,—an abundant code of signals might be arranged within the limits of not more than eight or nine blows in succession*.

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* The following may serve as an illustration of the method proposed for signalizing by means of blows. The number might be increased, and the signals modified, by employing scratchings with the point of a pick-axe, which can be heard at considerable distances, as well as by the method of blows.

Directions by the Experimenter observing the Deviations.		Answers or Inquiries by the Manager of the Magnets.	
No. of blows.	Indication.	No. of blows.	Indication.
2.	Attention.	2.	We are attending.
3.	Apply the magnet (N. pole) where you are; as near as you can tell opposite me, *at* the floor of the mine.	3.	It is done.
4.	*Reverse* the pole of the magnet, and apply it to the same spot.	4.	It is reversed.
5.	Neutralize the magnets.	5.	They are neutralized.
1, 2.	Apply the N. pole of the magnet to the middle of the working, at the floor of the mine.	1, 2.	It is so applied.
1, 3.	Carry the magnet to the *right* on the same level. N.B. The distance in yards to be indicated by the number of successive blows.	1, 3.	It is so applied.
1, 4.	Ditto to the extreme right of the mine; same level.	1, 4.	It is at the extreme right.
2, 1.	Ditto to the left,......so many yards.	2, 1.	It is so applied.
2, 2.	Ditto to the extreme left of the mine.	2, 2.	It is at the extreme left.
2, 3.	Raise the magnet vertically from the floor, so many feet.	2, 3.	It is raised as directed.
2, 4.	Raise it to the roof, directly upward.	2, 4.	It is at the roof.
3, 1.	Depress it to the floor, directly downward.	3, 1.	It is at the floor.
3, 2.	The experiment is finished.	3, 2.	Is the experiment finished?
3, 3.	The experiment is not finished.	3, 3.	I understand.
3, 4.	Do you understand?	3, 4.	I do not understand. What do you mean?

And by such a code it would be perfectly easy for the conductor of the experiment to give every requisite direction for the application of the magnets, at any distance to which the blows could be distinctly heard.

At distances, however, beyond the limits of communicable vibrations, the application of the magnets must be by preconcerted arrangement. In this case a certain moment of time might be agreed upon for the application of the magnets; and given intervals arranged when the magnets should be reversed, and when neutralized.

Though, in the preceding investigations, the equal permeability of solid substances generally has been verified or assumed, nothing has yet been stated in respect to the effect of the interposition of metallic iron on the action of the magnet. This substance, the action of which on the magnetic needle is so peculiar, has indeed been made the subject of very particular and diligent investigation, but the results have been intentionally omitted, both because these results are somewhat different from all the others, and because iron, in a metallic state, is not likely to be met with in mining, so as in any way to affect the universality of application of the process herein suggested for the measurement of distances otherwise indeterminable.

Thus much, however, may be briefly mentioned, that iron, in any state in which it can be had, whether malleable or cast, whe-

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4, 1. Is the magnet against wall?	4, 1. It is against the wall.
4, 2. Is the magnet neutralized?	4, 2. It is neutralized.
4, 3. Yes!	4, 3. Yes. It is.
4, 4. No!	4, 4. No. It is not.
1, 2, 1. Lay aside the magnet till directed.	1, 2, 1. May we lay aside the magnet?
1, 2, 2. Place the magnet.	1, 2, 2. Shall we place the magnet?
1, 2, 3. Which pole of the magnet is presented?	1, 2, 3. The North pole is presented.
	2, 1, 2. The South pole is presented.
2, 1, 3. Where is the magnet placed?	Answer by signal of other column.

ADDITIONAL SIGNALS TO BE FILLED UP.

2, 2, 1; 2, 2, 2; 2, 2, 3; 2, 3, 1; 2, 3, 2; 2, 3, 3; 3, 1, 2; 3, 2, 2; 3, 2, 3; 3, 3, 1; 3, 3, 2; 3, 3, 3; &c.

N.B. The separation of the number of blows by a comma, denotes a sensible pause between each series.

ther in steel tempered or soft, does not prevent the influence of the magnet being transmitted to the compass; but, on the contrary, I have most commonly found, that the deviations produced by the magnet acting through a mass of iron, are greater than when nothing whatever is interposed. I have also found that the influence is not interrupted, though the interposed metal be a mass of hardened steel, nor even when trial was made on a body of powerfully magnetic bars, two or three inches in solid thickness. When the mass of iron is not otherwise magnetic than as to its magnetism of position derived from the earth, then the action of a magnet passed through its neutral or equatorial plane, is, in all cases, (as far at least as can be inferred from very many trials), more energetic than when no such substance is near it. This was so particularly the case when the magnetic influence was passed through the two double cylinders and furnace of a locomotive steam-engine (consisting of ten successive plates of iron, forming a total of 5⅝ inches in thickness of metal), that it required the magnets to be a foot nearer to the compass in free space, than when placed beyond the engine, to produce equal deviations. For, whilst the distance of the magnets and compass, when the influence was transmitted through the engine, was 7 feet 8 inches, the distance on the open side, producing the same deviation, was 6 feet 7½ inches*.

Now this anomaly may be easily shewn to arise from the development of magnetism in the mass of iron, through the proximity of the magnets, by which the direct or permeable influence of the magnet is necessarily augmented; but that the whole of the effect which takes place is not due to this cause,—what is generally called "induced magnetism,"—but chiefly to the actual transmission of influence through the very substance of the mass of iron, I have distinctly and experimentally determined.

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* Some working engineers, who were present when this experiment was made, were exceedingly inquisitive about the effects produced, watching the deviations of the compass with great astonishment. One of them perceiving that the magnet, which was hid from his view by the engine, equally affected the compass as when presented in the open space, addressed himself to his comrade, and made this characteristic remark,—"Why, Tom," said he, "it sees through't!"

A Register of the Date of various Natural Appearances, kept at Treveroux Farm, in the Parish of Limpsfield in Surrey.

By Henry Cox, Esq. Communicated by W. Jacob, Esq. F. R. S. 36

Years.	Began sowing Oats.	Butterfly seen.	Bat seen.	Finished sowing.	Swallow first seen.	Cuckoo heard.	Black Thorn in Bloom.	White Thorn in Bloom.	Nightingale heard.	Hops polled.	Hops tied.	Began mowing.
1808.	Mar. 2.	Mar. 4.	April 1.	April 12.	April 9.	May 1.	May 1.	May 20.	April 13.	...	...	Cinquefoil, June 16.
1809.	Mar. 20.	Mar. 16.	Mar. 17.	April 27.	April 20.	May 4.	April 25.	May 15.	April 22.	...	...	June 24.
1810.	Mar. 10.	Feb. 28.	Feb. 28.	April 13.	April 18.	April 22.	April 30.	May 20.	April 21.	...	...	June 18.
1811.	Mar. 11.	Mar. 10.	Mar. 20.	Mar. 15.	April 21.	April 22.	April 20.	May 6.	April 13.	...	...	June 3.
1812.	Mar. 10.	Feb. 20.	April 20.	April 11.	April 21.	May 1.	April 27.	June 4.	April 16.	...	...	June 24.
1813.	Mar. 20.	Feb. 25.	June 4.	April 26.	April 26.	May 5.	April 15.	June 6.	April 15.	...	...	June 22.
1814.	Mar. 25.	April 2.	April 10.	April 12.	April 17.	April 20.	April 22.	June 1.	April 13.	...	...	June 28.
1815.	Mar. 4.	Feb. 9.	April 11.	Mar. 18.	April 13.	May 1.	April 6.	May 15.	April 12.	...	...	June 10.
1816.	Mar. 11.	Mar. 12.	May 23.	April 10.	April 23.	April 22.	May 12.	June 10.	April 19.	...	...	June 25.
1817.	Mar. 3.	Feb. 19.	Mar. 21.	April 2.	April 21.	May 4.	April 28.	June 8.	May 2.	...	...	June 21.
1818.	Mar. 30.	Mar. 29.	Feb. 14.	Mar. 31.	April 16.	April 25.	April 25.	May 23.	April 23.	...	...	Hay, June 18.
1819.	Mar. 17.	Mar. 15.	Mar. 24.	April 2.	April 6.	April 15.	April 10.	May 12.	April 10.	...	...	June 19.
1820.	Mar. 13.	Mar. 11.	Mar. 30.	April 22.	April 12.	April 18.	April 20.	May 22.	April 18.	...	...	July 3.
1821.	Mar. 22.	Mar. 11.	Mar. 16.	April 25.	April 16.	April 20.	April 20.	May 20.	April 20.	...	...	July 10.
1822.	Mar. 15.	Mar. 3.	Mar. 20.	Mar. 22.	April 21.	April 14.	April 10.	May 10.	April 21.	April 26.	June 20.	June 16.
1823.	Mar. 29.	Mar. 12.	Mar. 24.	April 3.	April 16.	April 21.	May 1.	May 25.	April 21.	May 2.	July 5.	July 7.
1824.	Mar. 31.	Mar. 17.	April 21.	April 20.	April 14.	April 25.	May 3.	June 5.	April 24.	May 6.	July 5.	July 5.
1825.	Mar. 24.	Feb. 21.	Mar. 20.	April 7.	April 14.	April 23.	April 26.	May 12.	April 19.	April 28.	June 18.	June 28.
1826.	Mar. 29.	Feb. 25.	Feb. 9.	April 6.	April 19.	April 22.	April 12.	May 10.	April 16.	April 26.	June 16.	June 26.
1827.	April 6.	Mar. 13.	Mar. 20.	April 10.	April 14.	April 14.	April 18.	May 14.	April 15.	April 29.	June 29.	June 25.
1828.	Mar. 25.	Mar. 9.	Mar. 10.	April 10.	April 13.	April 24.	April 19.	May 18.	April 16.	May 2.	June 30.	June 29.
1829.	Mar. 26.	Mar. 21.	Mar. 20.	April 9.	April 14.	April 23.	May 7.	June 3.	April 18.	May 7.	June 30.	July 15.
1830.	April 5.	Mar. 5.	Mar. 2.	April 8.	April 1.	April 15.	April 18.	May 15.	April 10.	April 24.	June 12.	July 5.
1831.	April 11.	Mar. 7.	April 10.	April 16.	April 14.	April 12.	April 20.	May 20.	April 12.	May 2.	June 20.	July 2.
1832.	April 2.	Mar. 9.	Mar. 23.	April 11.	April 10.	April 23.	April 25.	...	April 18.			...

Register of Natural Appearances—continued.

Years.	Finished Haying.	Wheat in Ear.	Began Reaping.	Finished Harvest.	Willow in Bloom.	Fresh Swarm of Bees.	Began Picking Hops.	Began Sowing Wheat.	Swallows' Flight.	Finished Sowing Wheat.	First Ice.	First Snow.
1808.	June 29.	June 24.	Aug. 5.	Aug. 26.	...	...	...	Sept. 21.	Sept. 25.	Nov. 8.	Nov. 6.	Nov. 19.
1809.	July 15.	July 1.	Aug. 10.	Sept. 2.	...	...	...	Sept. 15.	Sept. 20.	Nov. 18.	Oct. 12.	Nov. 4.
1810.	July 6.	June 28.	Aug. 14.	Sept. 5.	...	...	...	Sept. 30.	Oct. 1.	Nov. 20.	Oct. 2.	Dec. 30.
1811.	June 21.	June 10.	July 24.	Aug. 26.	...	...	...	Sept. 16.	Oct. 10.	Nov. 10.	Oct. 12.	Dec. 15.
1812.	July 8.	July 1.	Aug. 19.	Sept. 9.	...	...	...	Oct. 20.	Oct. 5.	Nov. 30.	Nov. 11.	Nov. 16.
1813.	July 4.	June 22.	Aug. 12.	Sept. 9.	...	...	...	Sept. 28.	Sept. 30.	Nov. 11.	Oct. 20.	Nov. 15.
1814.	July 13.	July 4.	Aug. 21.	Sept. 7.	...	...	...	Sept. 23.	Sept. 28.	Nov. 7.	Oct. 5.	Dec. 25.
1815.	June 24.	June 22.	Aug. 9.	Aug. 26.	...	...	...	Oct. 9.	Oct. 1.	Nov. 4.	Oct. 20.	Dec. 10.
1816.	July 31.	July 14.	Aug. 29.	Oct. 12.	...	...	...	Oct. 2.	Sept. 14.	Nov. 19.	Oct. 25.	Nov. 8.
1817.	July 14.	June 24.	Aug. 15.	Sept. 10.	...	...	...	Oct. 8.	Oct. 13.	Nov. 22.	Oct. 20.	Dec. 12
1818.	June 30.	June 8.	July 27.	Aug. 12.	...	...	Aug. 31.	Sept. 29.	Oct. 15.	Oct. 31.	Nov. 20.	None till Feb. 2.
1819.	July 24.	June 19.	July 31.	Aug. 25.	...	...	Sept. 8.	Oct. 1.	Oct. 6.	Nov. 19.	Oct. 21.	Oct. 22.
1820.	July 31.	June 25.	Aug. 9.	Sept. 13.	...	...	Sept. 13.	Oct. 2.	Oct. 1.	Nov. 6.	Nov. 20.	Nov. 15.
1821.	Aug. 13.	July 9.	Aug. 21.	Sept. 19.	...	...	Sept. 10.	Sept. 20.	Sept. 26.	Nov. 8.	Nov. 30.	None.
1822.	July 10.	June 10.	July 16.	Aug. 8.	Mar. 15.	May 5.	Sept. 2.	Sept. 27.	Sept. 28.	Nov. 14.	Nov. 17.	Dec. 25.
1823.	Aug. 15.	June 19.	Aug. 16.	Sept. 20.	April 1.	May 7.	Sept. 26.	Oct. 6.	Oct. 2.	Oct. 31.	Nov. 3.	Dec. 18.
1824.	Aug. 10.	June 26.	Aug. 16.	Sept. 30.	Mar. 30.	...	Sept. 14.	Oct. 18.	Sept. 27.	Nov. 1.	Oct. 12.	None till Mar. 10.
1825.	July 10.	June 20.	July 22.	Aug. 21.	Mar. 28.	May 4.	No Hops, 20th.	Oct. 15.	Sept. 27.	Oct. 26.	Oct. 26.	Dec. 27.
1826.	July 18.	June 21.	July 23.	Aug. 15.	Mar. 16.	May 16.	Sept. 4.	Oct. 4.	Sept. 20.	Nov. 3.	Nov. 7.	Dec. 6.
1827.	July 21.	June 24.	Aug. 3.	Aug. 28.	Mar. 23.	May 16.	Sept. 14.	Oct. 8.	Sept. 10.	Nov. 1.	Nov. 1.	Nov. 21.
1828.	Aug. 16.	June 20.	July 31.	Aug. 26.	Mar. 16.	June 6.	Sept. 15.	Oct. 8.	Sept. 5.	Oct. 28.	Nov. 8.	None till Jan. 7.
1829.	Sept. 1.	June 24.	Aug. 4.	Sept. 6.	April 4.	June 10.	Sept. 21.	Oct. 12.	Sept. 1.	Oct. 31.	Oct. 7.	Oct. 6.
1830.	Aug. 2.	June 26.	Aug. 9.	Sept. 1.	Mar. 26.	June 2.	Sept. 13.	Oct. 6.	Sept. 17.	Oct. 27.	Oct. 27.	Dec. 16.
1831.	July 27.	June 16.	July 29.	Aug. 22.	Mar. 22.	...	Aug. 29.	Oct. 15.	Sept. 20.	Nov. 3.	Nov. 10.	None.
1832.	...	...	...	...	April 4.	...	...	...	...	...		...

On the 19th of April 1808 there was a heavy fall of snow for four hours.

1808.—A fine productive harvest, and but little blight.

Last week in April 1809, very cold, wet, frosty, and unpleasant weather.

May 1809 came in very fine and hot.

1813.—An immensely productive harvest, and a general thanksgiving for it.

January 4th 1814, the deepest snow that has been known for 40 years began, and it continued on the ground for five weeks: at some places the drifts were 15 feet high. The frost continued 12 weeks to March 20. 1814.

1816.—From April 12th to 15th snow remained on the ground, and the weather was exceedingly cold and frosty.

1816.—September 3d, a hard frost which produced ice.

1817.—The month of August very wet, succeeded in September by fine harvest weather till November.

1818.—May 8th, a deluge of rain fell, after which no more rain fell at Treveroux or near, till September 5th, 17 weeks and one day, and all vegetation was completely burnt up.

1819.—October 22d, Snow six inches deep.

1822.—No rain from May 2d to July 5th, nine weeks of very hot days.

1823.—Rain little or much every day from June 29th to August 15th.—47 days.

1824.—A very wet summer, but not cold; crops of corn slight of hay heavy.

1825.—Sold the produce of 12 acres of hops for 5s.

1826 and 1827.—Two fine summers.

1828.—Very heavy rain every day from July 6th to August 14th.

1829.—Though the 9th of April is stated as the day on which oat-sowing was finished, yet an experiment was tried, by sowing white gate field with black Tartar oats, on the third of May. The weather was much against them at first, but they turned out very well, and were carried in on the 7th of October.

Rained more or less every day from the 16th of June to 20th September, except on four days, the 23d and 24th of July, and the 3d and 4th of September.

The season was not particularly cold, but was the wettest in my recollection.

1830.—Opened with a severe frost till February, followed by a warm dry March, without a storm or a shower. April 1st, a fall of snow till noon, whilst a swallow was seen flying about at Trevereux.

1831.—On the 6th of May occurred a most severe frost, the young shoots and leaves of the oak and ash were destroyed, fruit-trees of all sorts were greatly injured, and even the grass was checked to such a degree, that it never recovered from its effects.

Ice was nearly half an inch thick on the ponds on the common.

A severe frost, but inferior to the last described in its effects, occurred on the night of the 28th May 1818.

Earliest Knowledge of Gold and Silver.—Hesiod.—Scandinavian Museum.—The Patriarchs.—The Book of Job.—Accumulation of Wealth with the Hebrew Nation.—Accumulations in Syria and Persia; in Greece; in Rome.

In the earliest stages of society, so many and such great difficulties were opposed to the use of all metallic substances, that the discovery and application of them to the purposes of social life must have been slow and gradual.

The most ancient records of our race, the Sacred Writings, as well as the works of the earliest profane authors, have, however, communicated such intimations of the knowledge and adaptation of the more precious metals to the use of mankind, as tends to excite curiosity and to attract attention to the subject.

The general voice of antiquity affirms, that gold, silver, and copper, or brass (aes), were the first metals discovered; and that they were used partly as ornaments, and partly as instruments of war or of industry; for though, from their softness, they were not the best calculated for the latter purposes, they were better adapted to them than those implements of flint or other hard stones or hard wood, which had been before used by the most ancient tribes, and which were also found among the savage people inhabiting Australia, who were discovered in the middle of the last century.

A well-known passage in Hesiod affirms, that, in remote ages, "The earth was worked with brass, because iron had not been discovered;" and Lucretius bears testimony to the same purport, in book 5. l. 1286:

> "Et prior aeris erat, quam ferri, cognitus usus."

This is confirmed by the implements of copper found in the ancient mines, which will be hereafter noticed, in Siberia and Nubia; whose working must have ceased some thousand years ago.

When Brazil was first discovered by the Portuguese, the rude inhabitants used fish-hooks of gold, but had not iron, though their soil abounded in that metal. The people in Hispaniola and Mexico were, in like manner, unacquainted with iron when first visited by the Spaniards; though they had both ornaments and implements of gold, and weapons of copper, which latter, as we learn from the analysis of Humboldt, they had acquired the art of hardening by an alloy of tin.

This subject has been illustrated in Denmark, by opening many Scandinavian tumuli of very remote ages, from which have been collected specimens of knives, daggers, swords, and implements of industry, which are preserved and arranged in the Museum of Copenhagen. There are tools of various kinds formed of flint or other hard stone, in shapes resembling our wedges, axes, chisels, hammers, and knives, which are presumed to be those first invented. There are swords, daggers, and knives, the blades of which are of gold, whilst an edge of iron is formed for the purpose of cutting. Some of the tools and weapons are formed principally of copper, with edges of iron; and in many of the implements, the profuse application of copper and gold, when contrasted with the parsimony evident in the expenditure of iron, seems to prove, that, at the unknown period, and among the unknown people who raised the tumuli, which antiquarian research has lately explored, gold, as well as copper, were much more abundant products than iron.

Copper, in the more remote ages, was not only commonly, but in some, if not in all, exclusively used for money, and at these periods may be viewed as one of the precious metals; yet the changes that have since taken place have rendered gold and silver more entitled to that name, and will be so considered in the farther progress of this inquiry.

Some of the earliest notices which have reached the present day of the estimation of gold and silver, are in the account of the condition of Abraham, the progenitor of the Hebrew people, supposed to have lived two thousand years before our Christian era. We read, "that he was rich in cattle, and in silver, and in gold*." On the death of his wife, he purchased a field for a burying-place, the payment for which was made with four hundred shekels of silver, which he delivered not in coin, but "by weight, according to the currency of merchants†."

Joseph, the great grandson of Abraham, was sold by his brethren to a caravan of Arabs, travelling towards Egypt with the productions of their country, for twenty pieces of silver‡. Afterwards, when established in Egypt as minister of the king of that country, his brothers brought "silver in their sacks' mouths," to purchase corn during a season of scarcity in their native land. In the interesting sequel of the history of Joseph, when making himself known to his family, he presented to his younger and favourite brother three hundred pieces of silver§.

Though gold was known at that early period, and its value highly estimated, we find no intimation which can lead to the inference, that it performed the function of money, either by being used as the common measure of value for other commodities, or by being employed as the medium for exchanging one kind of goods for another.

The author of the Book of Job, whether, as some have supposed, a cotemporary of Abraham, or, as others have thought, of a date some hundred years later, is one of the oldest writers whose works have been transmitted entire to the present day. He was not only acquainted with gold and silver, but was accurately informed of the manner in which they were procured. "Surely," says he, "there is a vein for the silver, and a place for the gold where they fine it." He farther states, "that the

* Genesis xli. 2.

† Genesis xxiii. 14, 15, and 16.

‡ Genesis xxxviii. 29.

§ Genesis xlv. 22.

earth hath dust of gold*." Though living in a country which yields none of the precious metals, he was thus familiarly acquainted with the fact, that silver was found in veins, and gold commonly in small particles.

Among the people with whom Job was connected, silver seems to have passed from hand to hand by weight, as money; whilst gold was appropriated like the onyx, the sapphire, crystals, pearls, topazes, and other jewels, as ornaments for the person. At the conclusion of that beautiful poem, the restored wealth of Job is reckoned up in cattle, not in money; and though his visiters brought each a piece of money, probably silver, yet each of them brought also an ear-ring of gold†.

Of the Accumulation of the precious Metals, from the most remote Ages to the establishment of the Imperial Government in Rome.

There are no intimation in the Sacred Writings (Hebrews) which afford any means of forming an estimate of the whole quantity of the precious metals which had been collected in the patriarchal days. We must, therefore, rest satisfied with the scanty accounts with which they furnish, and proceed to later periods, when the relations of the several accumulations are more frequent, though not marked with any such precision as can inspire implicit confidence.

In the history of the reign of Solomon, as recorded in the Book of Kings and in the Chronicles, we find statements of the quantities of the precious metals used in the royal palace and the holy temple erected by that monarch. We read, that "he overlaid the house within with pure gold: and he made a partition by the chains of gold before the oracle, and he overlaid the oracle with gold. And the whole house he overlaid with gold, until he had finished the whole house: also the whole altar, that was by the oracle, he overlaid with gold‡."

The quantity of gold which Solomon collected in a single year, is stated to be (1 Kings x. 14), six hundred threescore and six talents, or perhaps about L. 300,000 in value at the present moment. That with which he covered the sanctum sanctorum, at the same rate, would amount to above L. 230,000.

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* Job xlii. 11. and 12.

† 1 Kings vi. 20, 21, and 22.

‡ Job xxviii. 1 and 6; also 15, 16, 17, 18, 19.

We learn from the Book of Kings*, that the king brought by his ships from Ophir four hundred and twenty talents of gold, or about L. 190,800. The Book of Chronicles† represents the amount greater as four hundred and fifty talents, or L. 203,000, a difference of no great moment, and one which, perhaps, a collation of manuscripts might reconcile.

Without attempting to calculate the quantity of metallic treasure heaped up by Solomon, we may best describe it in the language of his day. We read that "his throne was of ivory, overlaid with the best gold; that all the drinking vessels were of gold; that all the vessels of the forest of Lebanon were of pure gold: none were of silver, for that metal was nothing accounted of in the days of Solomon;" and, in short, "the king made silver to be as stones in Jerusalem †."

After this short intimation of the store of silver and gold accumulated by the Hebrew nation, it may be now proper to defer, to another branch of the subject, the consideration of the way in which such a store of the precious metals may probably have been collected under the reign of Solomon.

In proceeding from the sacred to the profane writers of antiquity, the reader is naturally in some degree surprised at the credulity, or at least apparent credulity, with which the most extraordinary and improbable tales are narrated. This is most remarkable in Herodotus and Diodorus, who are yet far from unworthy of confidence, where nothing supernatural is concerned. The Greek and Roman writers relate prodigies, which, at this day, we know not whether to attribute to their own credulity, or to that of the community for which they composed their works. In either case it does not render them utterly unworthy of credit, nor destroy their testimony in matter of history, of geography, of manners, of laws, or of government.

The history of all ancient nations is filled with prodigies which are no longer believed; but if, on that account, their authority on other subjects be discarded, it will become impossible to trace the progress of mankind through the several stages of society, from the most rude to the most civilized state. It is scarcely two centuries since in every part of Europe, with all the knowledge and civilization which had been imbibed, the belief in demoniacal possessions, in the power of witches and fairies, and in spectral appearances, universally prevailed. It would be unjust to the memory of the historians and chroniclers of that and the preceding ages, to reject their testimony, because they believed in supernatural events and appearances, which have lost all hold upon the present race, except among the most ignorant of the vulgar.

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* 1 Kings x. 28. † Chronicles viii. 18. ‡ Kings x.

If, in extracting from the writings of antiquity what relates more immediately to the subject of our inquiry, it should occasionally appear that incredible events are related, the sound judgment of the reader will enable him to separate the facts from the fictions in which they may be enveloped; and he may find amusement, if not instruction, in observing the great credulity of the eminent men of antiquity, and in comparing their habits of investigation and discrimination with those of the ablest writers of their own age and country.

Assyria and Persia.—It appears from the relations of Diodorus, that large masses of gold and silver had been collected together by Ninus, the founder of Nineveh, "who possessed himself of all the treasures of Bactriana, among which there was abundance of gold and silver*." From the same writer we learn that Semiramis, the wife of Belus, and the successor to his dominion, who built the city of Babylon, among other stupendous and almost incredibly magnificent works, erected in that city a temple to Jupiter, or Belus; "upon which were placed the statues of Jupiter, of Juno, and of Rhea, all of beaten gold. That of Jupiter was standing upright, was forty feet in height, and weighed a thousand Babylonian talents. That of Rhea was of the same height, sitting on a throne of gold, having a lion on each side of her, and one at her knee; and near them two vastly large serpents of silver, weighing 30 talents. The statue of Juno was in an erect posture, and weighed 800 talents. An altar was erected for their deities of beaten gold, 40 feet long and 15 broad, weighing 500 talents, upon which were two cups, each of them weighing 30 talents, and near to them as many censers, weighing 300 talents. There were also three drinking vases of gold, the largest of which was dedicated to

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* Diodorus, book ii. cap. 1.

Jupiter, weighing 1200 talents, and the other two 600 talents each [*]."

As Diodorus wrote in the Greek language, it is probable he adopted the weight of that country, according to which the value of this mass of gold, as calculated by the Abbé Barthelemy, would amount in our money to about L. 11,000,000 Sterling; whilst others estimate it at somewhat less, a difference, however, scarcely worth investigating. It is impossible not to suspect the statement here given of some exaggeration, though we may be induced to believe that a large quantity of the precious metals had been collected at that early period; but the exactness of the quantity must be a subject of doubt, when it is considered that Diodorus wrote near two thousand years after the events he relates, and in an age when written records must have been both rare and of doubtful authenticity. The probability of an accumulation of gold to a great extent in Babylon, is strengthened by the narrative in the book of Daniel, of the great size of the image of gold erected by Nebuchadnezzar, on the plain of Dura, near that city.

There is an appearance of authenticity and of accuracy in the account given by Herodotus of the tribute of gold and silver which Darius Hystaspes, king of Persia, about 480 years before Christ, drew from the several provinces, into which, after completing his conquests, he divided his extensive dominions. The amount supplied by each province is stated, and, whether paid in silver or gold, "the aggregate sum," he says, "will be found to be 9880 talents in silver, and estimating the gold at thirteen times the value of the silver; there will be found, according to the Euboic talent, 4680 of these talents. The whole being estimated together, it will appear, that the annual tribute paid to Darius was 14,560 talents [†]."

This treasure is estimated by Gibbon and by Rennel, to be equivalent to about L. 3,250,000 Sterling. According to the inference of the former writer, drawn from the same authority, in book i. cap. 192, this revenue was the surplus, after the expenses of the maintenance of the army, and of the provincial administration, had been discharged. This quantity of gold and silver, was probably that which became the property of the monarch, forming a kind of reserve stock, to meet unforeseen contingencies.

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[*] Diodorus, book ii. cap. 1.

[†] Herodotus, book iii. cap. 95.

We find in another passage in Herodotus, a description of the manner in which the treasure so collected was preserved in the royal residence. "The gold and silver were melted and poured into earthen vessels, and these when filled were removed, leaving the metal in a solid mass; when any was wanted, a piece was broken off, of the capacity which the occasion required[*]."

It seemed, as far as regarded Darius, to be the practice to coin no more gold or silver than was needed to conduct the commerce, and to defray the expenses of the state[†], which at that period could not be of any large amount, from the paucity of commodities, which were the subjects of exchange, and from the low prices which all the necessaries and conveniencies of life bore.

It is not improbable that this reserve fund was carefully kept from circulation by hoarding, as a preparation for the grand campaign against the Greeks, which must have formed a part of the warlike operations projected by the Persian monarch and his ministers. We learn that Xerxes took with him into the field so much money and valuable effects, as formed loading for 1200 camels[‡]; and, upon the disastrous events which attended his invasion, was under the necessity of distributing so large sums to the mercenary troops which had accompanied him to the field, that Sparta alone received from him 5000 talents[§].

Darius coined pieces of gold of great purity, which obtained the name of Darics: they were about the value of twenty-five shillings of our present money. The name Daric was at subsequent periods, however, given generally to all gold coins which contained but little alloy, and thus indicating the purity of the metal, rather than the weight of the piece. The darics of this coinage, were few in number, and contracted in circulation, or more of them would have been handed down to posterity. It is said, there are but two now known to exist, one of which is in the collection of Lord Pembroke. The figure of an archer is stamped on it, which gave rise to an ancient witticism, that may be worth relating.

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[*] Herodotus, book iii. cap. 96.

[†] Strabo, book xv. p. 505.

[‡] Demosth. de Symm.

[§] Isocrat. Συμμα 32.

Agesilaus, king of Sparta, received from Darius a bribe of 30,000 darics to withdraw from the other Grecian states with whom he was in alliance. Being reproached for his treachery, he defended himself, by asserting that his operations had been suspended, owing to his having been defeated by 30,000 archers.

Greece.—The wealth of Crœsus, king of Lydia, who lived about 540 years before Christ, has become proverbial; and, though no precise communication of the extent of it has been handed down, we may form some estimate of it, by the munificent present he made to the temple of Delphi, as related by Herodotus *, and Diodorus †, amounting to 4000 talents of silver, and 270 talents of gold, or near L. 3,000,000 in value of our money.

We find in Herodotus a story illustrative both of the wealth of this king, and of the manners of his time. When Crœsus sent his Lydians from Sardis to consult the oracle at Delphi, they were received with hospitality by the family of the Alcmæonidæ at Athens, and, on their return, acquainted their master with the kindness they had experienced. A member of that family received an invitation to visit Crœsus, and on his arrival was presented with as much gold as he was able to carry ‡. "To improve the value of the gift, Alcmæon made use of the following artifice: Providing himself with a large tunic, in which were many folds, and with the most capacious buskins he could procure, he followed his guide to the royal treasury; there rolling himself among the golden ingots, he first stuffed his buskins as full of gold as he possibly could; he then filled all the folds of his robes, his hair, and even his mouth, with gold dust. This done, with extreme difficulty he staggered from the place; and from swelling mouth, and projections all around him, resembling any thing rather than a man. When Crœsus saw him he burst into laughter, and not only suffered him to carry away all that he had got, but added to it other presents equally valuable. The family from this circumstance became exceedingly affluent, and Alcmæon was enabled to procure and maintain those horses which gained him the victory in the Olympic games."

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* B. i. c. 50.

† Diodorus, b. xvi. c. 56.

‡ Herodotus, b. vi. c. 105.

Whatever may have been the real wealth of Crœsus, it would appear that gold must have been of very high value in Athens, when such a load as an individual could carry about his person, in the way here described, could be sufficient to form the foundation of the fortunes of one of the aristocratical families of that state.

Pytheus, king of the petty territory of Celæna in Phrygia, has been celebrated for his wealth. According to Herodotus, he was a native of Lydia; but according to Pliny, of Bithynia; and it does not appear in what manner he became possessed of the rich territory he ruled. It is related of this man by Herodotus, that, "when Xerxes invaded Greece about 470 years before Christ, he entertained that monarch and his whole army with great magnificence; and being asked the amount of his wealth, replied to Xerxes, I conceal nothing from you, and will not affect ignorance, but fairly tell you the whole. As soon as I heard of your approach to the Grecian Sea, I was desirous of giving you money for the war. On examining into the state of my affairs, I found I was possessed of 2000 talents of silver, and 4,000,000, wanting only 7000 staters of gold of Darius; All this I give to you; my slaves and my farms will be sufficient to maintain me." According to the estimate of Larcher, an able French critic, the metallic treasures of this man, the ruler of a small territory, but the proprietor of rich mines of silver, amounted to L. 3,600,000 of our present money.

A long account of this man has been collected by Larcher, chiefly from the work of Plutarch, "De Virtutibus Mulierum." It narrates the measures taken by his wife to cure him of that passion for seeking gold, to which the lives of his subjects were sacrificed, and by which a want of sufficient food for subsistence was caused. As the story has been frequently told, and must be familiar to most readers, we may dispense with the relation of it. The application of the labour of all the inhabitants to the searching for, and purifying gold, caused such distress for food, that at length Pytheus was induced to direct, that only one-fifth, instead of the whole, of the citizens should in future be compelled to devote themselves to those operations.

The story of Pytheus is important to one of the objects of the present inquiry, inasmuch as it shows, as far as it regards the particular case, that the acquisition of gold and silver was only to be obtained, in that remote period, by the mines being in the hands of severe as well as arbitrary despots, who spared neither the enjoyments, the labour, nor even the lives, of their subjects, in the eager pursuit after the metallic riches of their dominions.

It does not appear that the free states of Greece possessed a store of gold and silver, equal to that acquired by these absolute rulers of smaller portions of territory. When Pericles *, in order to animate the Athenians, in their defence against the Peloponnesians, about the year 431 before Christ, addressed them, he stated the amount of the money then in the citadel to be L. 1,162,250; and, in addition to that, the gold in the statue of Minerva, which must be replaced if appropriated to the public service, to amount to L. 124,800. The revenues derived from the tributary states, amounted annually to the sum of L. 116,250, and more than L. 700,000 had been expended in improving the public works †.

The mass of the precious metals brought from the eastern to the western world by Alexander, must have been enormous, though much of that captured was expended in the subjugated countries, and in those which were between them and Greece. The accounts of historians are probably exaggerated; but whatever allowance may be made for such a practice which was too common with the ancients, we must be convinced from the numerous authorities ‡ which bear testimony to the facts, and corroborate each other, that the accumulation in the hands of individual monarchs and states, was much greater about the time of the establishment of the full power of the Roman empire, than any subsequent period.

The treasures acquired by Alexander in Susa and Persia, exclusive of those which were found in the Persian camp and in Babylon, are stated by the authors above referred to and others, by some at 40,000, by others at 50,000 talents. The treasure of Persepolis is rated at 120,000 talents; that of Pasagarda at 6000; and upon the capture of Ecbatana, according to the

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* Thucydides' Peloponnesian War, book ii.

† The sums here stated are taken according to the calculation of Dean Smith, the learned translator of Thucydides.

‡ Strabo, 615, p. 502.—Arrian, iii. 3. Justin. xi. 14, and Plutarch, Vit. *Alex.* 36.

account preserved in Strabo, 180,000 talents are said to have been collected from thence, besides 6000 talents which Darius had with him, which were taken by the murderers.

Alexander's profuse expenditure, which his flatterers called generosity, was in accordance with the vast sums he seems to have acquired. He gave great rewards to his soldiers, and paid their debts, amounting to 9800 talents. He presented to the Thessalians 2000 talents. The funeral of Hephæstion is said to have cost 12,000 talents, and the researches in natural history, for the work of Aristotle, 800 talents. The wealth of his satraps was also enormous. Harpalus, one of them, is said to have amassed 50,000 talents, although, when at Athens, he denied the possession of more than 950. The successors of Alexander also collected large sums; though, by their extensive and fierce wars, the greater part was dissipated.

In Polybius is found a description of Ecbatana, at a period subsequent to the capture of that place by Alexander, and afterwards in the reigns of Antigonus and Seleucus.

"The magnificence of the palace," he says, "was such in every part as to give a high idea of the power and wealth of those by whom it had been erected; for though the wood was all cypress or cedar, no part of it was left naked; yet the beams, the roofs, and the pillars that supported the porticoes and peristyles, were all covered with plates, some of silver, and some of gold. The tiles, likewise, were all of silver. Though the place had been three times plundered by those we have named before Antiochus arrived, there was still remaining, in the reign of Ena, some pillars cased with gold, and a large quantity of silver tiles, laid together in a heap. There were also some few wedges of gold, and a much greater number of silver. These were coined into money, and amounted to the sum of about 5000 talents *."

Ptolemy Philadelphus, the second king of Egypt after Alexander, is stated by Appian, upon the authority of official documents, to have possessed treasure to the enormous amount of 740,000 talents; either Roman talents, or the small Ptolemaic talent. If Roman talents, which were about equal to the Attic talent, be rendered into money of the present day, it will give the amount as 178,000,000. If the smaller talent, which seems most correct, be taken, it will amount to at least one-quarter of

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* Polybius, book v. cap. 9.—Hist. Rom. Proem. 10.

that sum. Though an account of this kind may appear exaggerated, yet there seems no reason to doubt its general veracity. The revenues of the Ptolomies were excessively large, and the countries over which their dominions extended were, by the collections, completely drained of all their wrought gold and silver; and the tributes were collected by the farmers of the revenue, with the assistance of an armed force, composed not of regular soldiers, but of organized bands of robbers. Some idea of the degree of rapacity in extracting revenues under Ptolemy, may be formed by comparing the tribute drawn from the provinces of Cœlesyria, Palestine, and Samaria, under Cyrus, as given by Herodotus, and that extorted by the successor of Alexander, as given by Josephus. At the time of Cyrus, the island of Cyprus was included in the province of Cœlesyria; but in the time of Ptolemy, was separated from it. In the first instance, the tribute paid was 350 talents *. In the latter instance, it was farmed to Evergetes for 8000 talents; but if the taxes were farmed by a Jew, he was to pay double that amount, and, moreover, supply to the royal treasury the money required to redeem the confiscated goods of such persons as had not paid their taxes †.

During the period of the Macedonian empire, the precious metals were spread in great abundance over the whole eastern shores of the Mediterranean; and if there had not been a very large portion of them hoarded up in the royal treasury, their value must have fallen much lower, in comparison with other commodities, than was actually the case ‡.

The Romans.—The extension of the Roman empire, until it comprehended almost the whole of the known world, if it tended to diminish the production of the precious metals, powerfully attracted them, from Asia and Africa, to its own metropolis. It is thus that the enormous fortunes of individuals, which are related by historians, are not to be accounted for. The descriptions of such fortunes, it is true, are not confined to their mere metallic wealth, but include their lands, houses, slaves, and furniture, and also money lent at interest on mortgages or other securities. But unless the metallic wealth had increased in a prodigious degree, that remarkable rise in the prices of other commodities could not have been experienced, which is noticed by all writers. As, among other instances, we know that the house of Marius[*], at Misenum, was purchased by Cornelia for 75,000 drachmas[†], and a few years after sold to Lucullus for 500,200 drachmas[‡]. The fortunes of private individuals may be judged of by a few select notices to be found in contemporary authors. Crassus is said to have possessed, in lands, bismillies[§], besides money, slaves, and household furniture, estimated at as much more[||]. Seneca is related to have possessed termillies[¶]. Pallas, the freedman of Claudius, an equal sum. Lentulus, the augur, quatermillies[**]. C.C. Claudius Isidorus, although he had lost a great part of his fortune in the civil wars, left, by his will, 4116 slaves, 3600 yoke of oxen, 257,000 head of other cattle, and, in ready money, H. S, sex centies[††].

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* Herodotus, book iii. cap. 89.

† Josephus Antiq. Jud. xii. 4.

‡ See, on this subject, the valuable German work of Professor Boeckh, entitled, "Staatshaushaltung der Athener," an excellent translation of which was published in 1828, under the title of "Public Economy of Athens."

The emperors were possessed of wealth in a proportion commensurate with their superior rank and power. Augustus obtained, by the testamentary dispositions of his friends, quaterdecies millies[‡‡]. Tiberius left at his death vigesies a septies millies[§§], which Caligula lavished away in a single year.

The expenses of the government, and the debts and credits of the most eminent individuals, seem to have been on the same colossal scale. Vespasian, at his accession, estimated the money which the maintenance of the commonwealth required at L. 322,916,660.

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[*] Plutarch in Mario.

[†] L. 2421 : 17 : 6 Sterling.

[‡] L. 16,152 : 5 : 10 Sterling.

[§] L. 1,614,583 : 6 : 8 Sterling.

[||] Though Crassus had several silver mines, and estates of great value, which were profitably managed, yet his revenues from those sources are represented as inconsiderable, when compared with those he derived from his slaves. He had a large number of them, whom he educated, who were taught to become readers, amanuenses, book-keepers, stewards, and cooks. Besides this, he made interest of his money, at a high rate, receiving for the use of it one per cent. at the end of each month. It is recorded, as a saying of his, "that no man could be accounted rich who was not able to maintain an army out of his own revenues." It would seem, that when he was desirous to form a powerful party in the state, he could be occasionally as profuse as he was habitually avaricious; for on one occasion he gave an entertainment to the populace, who were seated at 10,000 tables, and at another time gave them a supply of bread-corn for three months.—Plutarch, Life of M. Crassus.

[¶] L. 2,421,875 Sterling.

[**] L. 3,229,166 Sterling.

[††] L. 484,375 Sterling.

[‡‡] L. 32,291,666 Sterling.

[§§] L. 21,796,875 Sterling.

The debts of Milo amounted to H. G. septengenties *. Julius Cæsar, before he held any office, owed 1300 talents. When, after his prætorship, he set out for Spain, he is reported to have said, "Bis millies et quingenties sibi deesse, ut nihil haberet;" that is, that he was L. 2,0018,000 worse than nothing. When he first entered Rome, at the beginning of the civil war, he took out of the treasury to the amount of L. 1,095,000 Sterling, and brought into it, at the end of that war, L. 4,843,000. He is reported to have purchased the friendship of Curio, at the commencement of the civil contests, by a bribe of L. 484,370; and that of the Consul L. Paulus, the colleague of Marcellus, by one of L. 279,500 †.

Anthony, on the ides of March, when Cæsar was killed, owed L. 320,000, which he paid before the kalends of April, and squandered of the public money more than L. 5,600,000 ‡.

Many other instances might be found of vast masses of wealth being collected, of large debts being contracted, and of prodigious sums being expended, either on public occasions, or in private indulgences of the dress, the tables, or the furniture of the Romans, just after the acquisition of universal empire. At that period the treasure, which had been acquired by conquest, had not been generally in the hands of numerous individuals, nor had much of it been consumed by the friction, which the practice, soon after extended, of converting large quantities of it into coined money, necessarily occasioned.—*Jacob on the Precious Metals.*

On the Origin and Composition of Basalt.

BASALT, like granite, appears composed of several different minerals, and has not derived its existence from the fusion of granite or any other known rock. With the view of testing in some degree the accuracy of this opinion, Leonhard requested of the celebrated chemist C. G. Gmelin, who has published so interesting an account of the composition of Phonolite or Clinkstone, to examine basalt in the same manner as he had phonolite. Leonhard, in his great work on Trap-rocks, now in the press, tells us that Gmelin readily agreed to undertake the analysis, and had already communicated to him the following examination of basalt.

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* L. 565,104 Sterling.

† It is remarked by Pliny (book xxxiii. cap. 3), that the city of Rome never possessed so much money as at the beginning of the war between Cæsar and Pompey.

‡ See Adam's Roman Antiquities, 9th edit. p. 461, from whence, as far as regards Rome, the facts are selected, and where the evidence on which each of them rests is pointed out.

Analysis of Basalt, by Professor C. G. Gmelin.

The analysis was conducted in the same way as that of phonolite.

Basalt from Stetten, a Conical Basaltic Rock in Hegau.

100 parts of the gelatinizing mass contains	
Silica,	35.741
Alumina,	11.121
Oxidulated Iron,	16.015
Oxide of Manganese,	1.487
Lime,	11.914
Strontian,	0.112
Magnesia,	10.434
Natron,	3.264
Potash,	1.204
Water,	6.530
	97.822

100 parts of the not gelatinizing mass contains	
Silica,	48.500
Lime,	17.395
Magnesia,	13.131
Alumina,	6.792
Oxide of Iron,	9.383
Oxide of Manganese,	0.436
	95.637

Contents of the analyzed Basalt,	10.162 grains.
.... gelatinizing mass,	6.254
.... not gelatinizing,	3.908

Contents of the Basalt altogether,

Silica,	40.64
Alumina,	9.57
Lime,	14.02
Strontian,	0.07
Magnesia,	11.47
Oxidulated Iron,	13.35
Oxide of Manganese,	1.10
Potash,	0.74
Soda,	2.01
Water,	4.01
	96.98

The mass which does not gelatinize yields, when what appears to be magnetic iron is abstracted from the oxidulated oxide of iron,

Silica,	44.50
Alumina,	13.85
Lime,	14.83
Strontian,	0.14
Magnesia,	13.00
Soda,	4.06
Potash,	1.49
Water,	8.13
	100.00

This result does not agree precisely with that of any other mineral, with respect to the component parts, but it approaches, in some measure, to anorthite, a species of felspar discovered by G. Rose. Anorthite is entirely decomposed by concentrated muriatic acid. It likewise contains a considerable quantity of magnesia, but what particularly deserves notice is, that no other fossil containing much magnesia gelatinizes with acid. The quantity of silica and lime in that part of basalt which gelatinizes with acids, agrees completely with that contained in anorthite. But, on the other hand, anorthite contains a far greater quantity of aluminous earth, considerably less magnesia, and no alkali. On the whole, it appears that that part of basalt which gelatinizes with acids, is the regular mass from which the various crystals are developed, that occur so frequently in basalt. For example, it is easy to perceive, that in consequence of the disappearance of magnesia, Labrador felspar, that universal component part of dolerite and syenite, as well as of many meteoric stones, stilbit so frequent in basalt, as well as chabasie, prehnite and arragonite, will be found in the mass.

The portion which does not gelatinize has nearly the same component parts as augite.

The analysis of basalt from Hohenstoffeln in Hegau is not yet quite completed. The proportion of the gelatinizing part to that which does not gelatinize is = 6.197:3.303, and it deserves to be noticed, that it approaches very nearly to the basalt from Stetten.

100 parts of the gelatinizing portion contains,—

Silica,	35.13
Alumina,	12.24
Oxidulated Oxide of Iron,	15.30
Oxide of Manganese,	1.70
Lime,	8.08
Magnesia,	13.07
Soda,	3.05
Potash,	1.91
Water,	4.59
	98.17

Without doubt that part which does not gelatinise is completely analogous in its composition to the basalt from Stetten.

The gelatinous portion of basalt from Sternberg near Urach, has likewise the greatest analogy in its composition with the preceding. In this basalt the portion that gelatinizes is very remarkable: its proportion to that which does not gelatinize = 100.14.

100 parts of the gelatinizing mass consists of—

Silica,	36.94
Alumina,	10.58
Oxide of Iron,	13.34
Oxide of Manganese,	0.30
Lime,	14.18
Magnesia,	11.04
Soda,	3.30
Potash,	2.46
Water,	3.59
	95.73

Basalt from the vicinity of Wezlar.

This basalt exhibits a very distinct decomposition on its surface, consisting of two to four lines of bluish-grey, and where the decomposition has proceeded farther, of a light yellow colour. In this instance, the decomposed part, as well as that which is undecomposed, requires examination. Of the former, a part of that which was most completely decomposed was employed. The principal result was anticipated, as might be expected, namely, that the decomposition diminished the relative proportion of the gelatinizing mass.

The gelatinizing mass in proportion to that which did not gelatinize,	The undecomposed Basalt,	= 067,47·1
	The decomposed Basalt,	= 031,10.1

100 parts of the gelatinizing mass of the undecomposed basalt contains—

Silica,	28.9
Titanium,	6.63
Alumina,	11.64
Oxidulated Oxide of Iron,	28.79
Oxide of Manganese,	0.21
Lime,	7.37
Magnesia,	5.46
Soda,	3.67
Potash,	1.50
Water,	5.32
	99.50

The portion which did not gelatinize exhibited a peculiar composition; but the analysis must be repeated, as the loss is so considerable:—

Silica,	56.65
Alumina,	9.16
Oxide of Iron,	3.99
Lime,	12.65
Magnesia,	3.91
Soda,	3.17
Potash,	3.13
	92.66

The quantity of alkali is remarkable, and it might be supposed that it was acquired in consequence of the silica in the gelatinizing portion being separated from that which did not gelatinize by the carbonate of potash; but that cannot be the case, because the powder was first of all acted upon by diluted muriatic acid, and then carefully washed.

The result of the investigation of the decomposed mass was, that the quantity of silica, titanium, lime, magnesia, and the alkalis was diminished, while that of the oxide of iron was considerably increased.

Chrome has been found in all analyzed basalt; without doubt it is contained in olivine, and perhaps the chromate of iron is likewise to be found. Professor Bugengeiger of Freyburg has long since discovered chrome by means of the blowpipe.

Nothing was discovered after repeated experiments with muriatic acid, nor with sulphuric acid. Lithia has not been discovered. Strontian was by no means always found; but it was decidedly discovered in the basalt from Stetten.

On the Cholera Animalcule.

It is a very generally diffused opinion, although supported by no positive facts, that those animated creatures belonging to the lowest classes to which, on account of their minuteness, the name of Microscopic Animalcules has been given, are formed by the simple aggregation of the so-called organic molecules; and Dr Hermann has endeavoured to explain the contagious nature of cholera upon this supposition. As similar views have been more than once suggested, and it is to be feared that their plausibility may gain for them a more extended credence; the opinion of a naturalist deeply versed in microscopic inquiries, and who has personally observed the oriental plague, a disease not dissimilar in some of its characters to cholera, merits consideration. Professor Ehrenberg, in a late fugitive piece, has expressed himself in the following terms upon this subject.

To the doctrine of the similarity of the contagion of plague and of cholera, is connected with another which has lately found its way into the public journals, and which is merely a revival of the old and antiquated idea of small invisible insects which generate this contagion by their irritation, poison, &c., and propagate it by their increase and migrations. Similar stories are to be found in the traditions of various people as well as those of the poisonous look of some human faces, of the dragon, of witches, magicians, the second sight, &c., formerly so seriously believed, but now only thought ridiculous. Linné, the great reformer of natural history, first took this fabulous animalcule into the domain of natural history, probably only with the view of directing the attention of naturalists to the subject.

It was said to be the cause of the pestilential blisters of the Gulf of Bothnia. He gave it the vermiform shape, and the yellow colour of northern tradition, and conferred on it the scientific name, more ridiculous than formidable, of the Infernal Fury (*Furia infernalis*). Before that, at the time of the plague at Marseilles in 1721, the contagion had been ascribed to small infusory-like winged or mitelike, yet invisible, animals; and at the time there appeared in the French language many treatises, which must now appear absurd to every well-informed person. One of these, printed anonymously in 1726, to push the matter still farther, deduces all diseases from these animalcules, which are designated by the following name: *Vers assoupissans, cours de ventristes, barbon quifians, clouifians, erectifs, fistulaires lacrymaux, fleuistes blancs!* The tradition of the Linnean Furia still remains in Finland, where the anthrax is common; and, in Siberia, I found, in 1829, on my journey with Baron von Humboldt, a similar tradition regarding the cause of the Siberian pestilential boil, only that it was ascribed to flying large insects, without, however, one of them ever having been exactly characterised or even taken. Although we passed through many places infested with the pest, and I neglected no opportunity of learning the causes of the disease, I found no trace of this insect.

A similar tradition gave rise to the question which was put to Dr Hemprich and myself, in the year 1823, by the Pacha of Egypt, whether it was true that, in Dongala, there were flying scorpions which produced mortal wounds, for the troops refused to march there, having already suffered much from those without wings. As during my natural history researches for nine months in Dongola, I had found nothing which justified this belief, except the troublesome small mosquitoes, which were neither poisonous, nor scorpions, the mind of the Pacha was set at ease.

As was to be expected, the same idea of invisible poisonous insects was transferred to the contagion of cholera; yet it is hardly credible that Hahnemann, as stated in the Leipzic Journal, should have for this reason recommended the sedative effects of camphor, because it killed these insects, and so expelled the cholera.

I have, for many years, made these minutest of organic bodies the subject of my particular inquiries, and have for that purpose employed the best instruments. But none before me, nor have I myself, ever succeeded in finding in the air these small bodies to which tradition had given a real existence. I must, therefore, warn medical men from modes of treatment of cholera founded upon this principle, for no naturalist has yet observed these animalcules. I have never observed these animalcules under the microscope, at the time of the plague in Egypt and Siberia; and previous to my African journey, in the Hospital of the Charité at Berlin, I had examined with the microscope many contagious cutaneous eruptions, without having ever seen them. While, by the most rigorous microscopic accuracy, I have made the singular discovery, that infusory animalcules, from $\frac{1}{8}$th to $\frac{1}{2000}$th of a line in size, possess an organization similar to many of the higher animals, and have demonstrated their propagation by eggs and internal organs, which are less than $\frac{1}{36000}$th of a line, or $\frac{1}{432000}$th of an inch in diameter, and are yet distinctly visible.

What must, then, be the size of the pest or cholera animalcules, or *cours de ventristes*, if they were not discernible by such instruments? The opinion is to be classed in the same rubric with the traditions and hypotheses of dragons, &c., and has at least been confirmed by the experience of no credible naturalist.

According to the observations of Professor Ehrenberg, the so called "Priestley's Matter," when it is not formed by real animals of a very different form, was by algæ; and particularly when it appears as a pellicle or cuticle, is the result of putrefaction, and only consists of the dead bodies of infusoria. It is therefore not the commencement of new formations, but the remains of dead organic generations.

On the Crystallization of Ice, and of Veins of Ice in Ice. By Professor Hessel.

For some time past I have been occupied with observations on the different forms of crystallization. The crystallization of water under certain conditions, induced by artificial means, formed also the subject of my inquiries. I shall here briefly detail one of my experiments, which I have repeated frequently of late, as I reckon it not unimportant for the doctrine of veins, whose different modes of origin can, in my opinion, only be satisfactorily explained by collecting as many examples as possible of the formation of veins and vein-like masses since the commencement of historical epochs. So that we have then only to inquire whether this or the other vein, or assemblage of veins, bears most resemblance to lava-veins in lava, to veins which may be considered as canals filled up by mineral springs of some sort or other, to fissures filled by sublimations, to fissures which have been the outlets for alternate streams of fluid or elastic matters, and which have been gradually closed by the deposition of solid matters, or to fissures which have been filled by infiltration from above, &c.; or whether these veins are to be viewed as the result of the contemporaneous congelation (crystallization) of two or more heterogeneous masses, one of which has filled fissures in the other, but which have never been in reality open.

Upon this supposition every experiment on the origin of vein-like masses, however insignificant it may appear, must be considered as an augmentation of our resources for the elucidation of the origin of those veins which have not been observed by man, so that this communication is of interest not merely to the crystallographer but also to the geognost.

I set aside, in a warm room, a mixture of fine clay and water, in which the latter was somewhat in excess, so that the thin mud could be easily stirred about with a fine hair-brush. Upon resting for some time it divided into two portions, the undermost of which consisted of moist clay, and the upper and least considerable of clear water. During the cold days which we had in December (5° — 10° F.), I exposed the mixture after agitation to crystallization or freezing. Crystallization did not take place till the mass had returned to the state of rest, but before the separation took place between the clay and the water. The structure of the frozen mass varied in different experiments. In every case, however, frozen mud and frozen clear water could be distinguished from each other. But the latter did not occur as a stratum at the upper part of the mass, but was distributed through the substance of the frozen mud.

1. The most common appearance was like that of small quartz veins traversing in different directions a siliceous slate. The same as in hand specimens of siliceous slate, when two of the quartz veins meet one another, they traverse one another, shift one another, or mutually cut each other off, &c. was observed distinctly in the present instance. The principle that the traversing vein is newer than the one traversed, could not be easily demonstrated to be correct in these ice veins in the frozen mud; nor could the idea of the contemporaneous formation of veins with the surrounding rock be admitted as unconditionally correct. In these ice veins there was apparently a real cutting across of one vein by another, so that the traversed vein beyond the traversing pursued its original course, or was diverted somewhat from its position, but more frequently one was completely cut off by the other. Often we could suppose a true wedging out of such a vein without a previously existing empty fissure promoting its formation.

2. Often the water-ice was distributed through the frozen mud like the quartz in the felspar of graphic granite. The surface formed by cutting and polishing exhibiting, like the latter Hebraic, Arabic, and Chinese characters; and these were still more characterised on the dark surface of the mud, than the greyish-white quartz on the whitish felspar.

3. Another mode of distribution of the water-ice in the frozen mud, was its forming vertical plates, which were so grouped that the surface of the mass of mud on its middle section, resembled a concentrically radiated crystalline mass, the rays diverging from the centre outwards. Several of these groups of rays were observed. Each ray projected to a considerable height above the surface of the mud.

During the formation of the veins, I sometimes observed also that of hollow spaces. These were enclosed by three or more ice veins, which penetrated obliquely downwards, partly to the bottom of the vessel; they had a breadth of from half to three quarters of an inch, and were quite empty of the frozen mud.

The frozen mud, separated as much as possible from the transparent ice, gave, when thawed, a moist mud, so that crystallization had produced no more complete separation than simple rest; but the ordinary separation of the water from the mud was produced in a much shorter time than by the mere operation of specific gravity in the mechanical mixture.

With regard to the causes of the three different appearances that I have enumerated, they appear to me to depend upon differences in the excess of water in the mud, on the temperature of the mass before it is exposed to congelation (sometimes boiling water was used), and especially the rapidity of the congelation. Farther I can give no explanation.

Account of the Introduction of the Wood-Grouse or Capercailzie (Tetrao Urogallus) to the Forest of Braemar. By JAMES WILSON, Esq. F.R.S.E., M.W.S., &c. Communicated by the Author.

THE almost recent extinction in Britain of the largest European bird of the gallinaceous order, is a remarkable fact in the geographical history of the species. Its reintroduction is also a circumstance of sufficient interest to deserve a detailed record.

The wood-grouse or capercailzie, was formerly a well-known and frequent inhabitant of the Scottish forests. It still occurs in considerable abundance among the wooded and alpine districts of Europe, especially in Scandinavia. It is rare in France, well-known in Germany, not unfrequent in Switzerland. It spreads through Russia into Siberia, and is very numerous in several districts of the north of Asia. It seems always to prefer mountainous forests, and is rarely met with in plains or flat countries, however richly wooded. Its favourite trees are pines, birch, and juniper. It feeds on the fruit of the last-named plant, and on the buds and tender sprays of the two former. Colonel Montagu found the crops of two females which he examined, to contain a species of berry similar to the cranberry, called in Norway *Tytteboer;* and the tops of that plant, together with sprigs of the common heath, appeared to have been swallowed in considerable quantity. The gizzard was extremely strong and muscular, and contained a large mass of pebbles intermixed with the macerated food*. Many other alpine and woodland plants, no doubt, minister to its wants, and, in common with the rest of its order, insects of various kinds may be presumed to be sought after, especially by the young.

These birds are of polygamous habits, and consequently do not pair. During the breeding season, which commences as soon as the buds begin to expand, and continues throughout the rapid northern spring till the forests are clothed in their freshest green, the male is frequently seen perched on some tall pine, where he moves backwards and forwards, uttering at the same time a peculiar cry, which seems to attract the neighbouring females. His head, on these occasions, is red and swollen; his wings dependent, and his neck extended. His cry is said to commence with a loud explosion, which is followed by a noise like that of the whetting of a scythe. This is heard at a great distance, and, as soon as the females are collected around the tree, the male descends from his "high estate," and joins their company†.

The last capercailzie recorded to have been killed in Scotland, was shot, about fifty years ago, near Inverness. For a considerable time anterior to that period, it had been of extremely rare occurrence, and, although a solitary remnant of the ancient stock may have contrived to maintain a precarious existence for a few succeeding years in some obscure recess of the umbrageous forests of Braemar or Rothiemurchus, it can scarcely be doubted that the species, ere long, ceased to exist as indigenous to Britain. It was known to have been extirpated from Ireland at a considerably earlier period.

When we consider the great size and beauty of this species of game, and its value as an article of food, we need not wonder that various attempts have been made to naturalize it for the second time in Scotland. I shall confine my present notice to the individuals which I have myself had an opportunity of observing.

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* Supplement to the Ornithological Dictionary.

† Journal Economique, April 1753.

I had last summer the pleasure of accompanying my scientific friends, Professor Graham and Dr Greville, on a botanical excursion to the Valley of Clova. The discovery of *Astragalus alpinus*, till then unknown as a British plant, and of other interesting rarities, rewarded their zeal, and has been elsewhere recorded*. For myself, I chiefly plied the angler's trade, and had the satisfaction of providing my friends and their followers (Professor Graham being accompanied by a detachment of his class) occasionally with an agreeable addition to their dinner in regions where there were very few loaves, and (but for my exertions) no fishes. We afterwards crossed the Grampians, skirting the "dark Loch-na-gar" and other fine mountain masses of that neighbourhood, and, descending to the banks of the Dee, took up our residence for a time at the Castletown of Braemar.

I was wading down the Dee one fine afternoon, a little below Mar Lodge, and with a lighter pannier than usual, when I heard the cry of a bird to which I was unaccustomed, and my bad success in that day's angling induced me the more readily to diverge from the "pure element of waters," to ascertain what this might be. I made my way through the overhanging wood for a few hundred yards, and soon after reaching the road, which runs parallel with the river on its right side, I observed a wooden palisade, or enclosure, on the sloping bank above me. On reaching it, I found it so closely boarded up, that I had for a time some difficulty in descrying any inmates, but my eye soon fell upon a magnificent bird, which at first, from its bold and almost fierce expression of countenance, I took rather for some great bird of prey than for a capercailzie. A few seconds, however, satisfied me, that it was, what I had never before seen, a fine living example of that noble bird. I now sought the company of Mr Donald Mackenzie, Lord Fyfe's gamekeeper, the occupant of the neighbouring cottage. He unlocked the door of the fortress, and introduced me to a more familiar acquaintance with its feathered inhabitants. These I found to consist of two fine capercailzie cocks and one hen, and the latter, I was delighted to perceive, accompanied by a thriving family of young birds, active and beautiful. I made various inquiries on the spot; but the fatigues of angling, and of entomologising combined, prevented my writing down the result at the time, although I have still a distinct recollection of the leading facts*.

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* See this Journal, October 1831, p. 373.

It was, however, with great pleasure that I availed myself, at an after period, of Sir Thomas Dick Lauder's obliging offer to convey a series of queries to Mr Cumming, Allanquoich, Braemar, Lord Fyfe's factor, from whom I received the substance of the following information.

The first importation of these capercailzies arrived from Sweden about the end of the year 1827, or early in January 1828. It consisted of a cock and hen, but the hen unfortunately died after reaching Montrose Bay. As the male bird alone arrived at Braemar, the experiment was judiciously tried of putting a common barn-door fowl into his apartment during the spring and summer of 1828. The result was, that she laid several eggs, which were placed under other hens, but from these eggs only a single bird was hatched, and when it was first observed it was found lying dead. It was, however an evident *mule*, or hybrid, and shewed such unequivocal marks of the capercailzie character as could not be mistaken.

The second importation likewise consisted of a cock and hen, and arrived safely in this country in January or February 1829. The female began to lay in the ensuing April, and laying in general an egg every alternate day, she eventually deposited about a couple of dozen. She shewed, however, so strong a disposition to break and eat them, that she required to be narrowly watched at the time of laying, for the purpose of having them removed, for otherwise she would have destroyed the whole. In fact she did succeed in breaking most of them, but eight were obtained uninjured. These were set under a common hen, but only one bird was hatched, and it died soon after. In the spring of 1830, the hen capercailzie laid eight eggs. Of these she broke only one, and, settling in a motherly manner on the other seven, she sat steadily for five weeks. On examining the eggs, however, they were all found to be addle. "It is to be remarked," Mr Cumming here observes, "that in 1829 and 1830 the hen had access only to the cock that was brought home with herself."

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* During our excursion we generally passed over the ground more rapidly than was consistent with entomological observation. The objects sought for by the botanist are generally of larger size, and being also lovers of light and sunshine, they are more easily distinguished than most of the insect tribes, so many of which court concealment and the shade. I was, however, fortunate in obtaining near the Spittal of Glenmuick the scarce heath-butterfly *Hipparchia Typhon*, which I had never before seen alive in Scotland; and in open glassy glades, among the woods which skirt the right bank of the Dee, between Abergeldy and Invercauld, I captured the rare and beautiful *Hipparchia Blandina*, commonly called the Scotch Argus, a species hitherto found chiefly in the island of Arran, and not previously known to occur so far north on the mainland. Of the rarer Diptera, *Pedicia rivosa* may be mentioned as not unfrequent among the woods of Braemar.

In the early part of 1831, three apartments were ingeniously formed adjoining one another. The hen was placed in the central chamber, between which and the enclosure on either side, each of which contained a male, there was an easy communication; so contrived, however, that the female could have access to both the males, whilst they, from their greater size, could neither approach each other, nor disturb the female as long as she chose to remain in her own apartment. In May and June of that year she laid twelve eggs, seven of which were set under a common hen. Of these, four were hatched in an apparently healthy state, one was addle, and the other two contained dead birds. Of those left with the capercailzie hen, she broke one, and sat upon the other four, of which two were hatched, and the other two were found to contain dead birds. Of the two hatched one soon died. Both the barn-door hen and the female capercailzie sat twenty-nine days, from the time the laying was completed till the young were hatched; and Mr Cumming calls my attention to the fact, that there were birds in all the eggs of this year's laying except one.

My visit to Braemar took place about the first week of last August. I think all the five young were then alive, and although only a few weeks old, they were by that time larger than the largest moor-game. I had no opportunity of handling them, or of examining them very minutely, but the general view which I had of them, at the distance of a few feet, did not enable me to distinguish the difference between the young males and females. They seemed precisely the same at that time both in size and plumage, although I doubt not the male markings must have soon shewn themselves on the young cocks. The single surviving bird of those hatched by the mother died of an accident, after living in a very healthy state for several weeks. Two of those hatched by the common hen died of some disease, the nature of which is not known, after lingering for a considerable time. It follows that there are only two young birds remaining. These are both females, and when I last heard of them some months ago, were in a thriving condition.

The whole progeny were fed at first, and for some time, with young ants,—that is, with those whitish grain-shaped bodies, which are the larvæ and crysalids in their cocoons of these industrious creatures, though commonly called ant's eggs. At that period they were also occasionally supplied with some tender grass cut very short. As soon as they had acquired some strength, they began to eat oats and pot barley, together with grass and the various kinds of moss. They are now fed like the three old birds, chiefly on grain and heather tops, with the young shoots, and other tender portions of the Scotch fir. I am informed that the distinction between the sexes had become very obvious before the death of the young males. The plumage of the latter was much darker, their general dimensions were greater, their bills larger and more hooked. These characters became very apparent during November and December.

The old males have never yet had access to the young birds, so that it has not been ascertained whether they entertain any natural regard for their offspring, or would manifest any enmity towards them. From the continued wildness of the old birds, especially the males, it was found difficult to weigh them, without incurring the risk of injuring their plumage. However, the male which arrived in 1829, and which then appeared to be a bird of the previous year, was lately weighed, and was found to be eleven pounds nine ounces avoirdupois. Judging from appearances, it is believed that the weight of the old hen would not much exceed one half. There is, indeed a striking disparity in the dimensions of the sexes in this species.

I have not yet heard the result of this season's courtship. The intention is, as soon as some healthy broods have been reared in confinement, to liberate a few in the old pine woods of Braemar, and thus eventually to stock with the finest of feathered game the noblest of Scottish forests.

WOODVILLE, *6th June* 1832.

Catrine Works Meteorological Register for 1831.

1831.	ATMOSPHERIC VARIATIONS.				STATE OF THE WIND TAKEN AT NOON.								ATMOSPHERIC PRESSURE.			Attached Thermometer.			External Thermometer in Shade.			FALL OF RAIN.	
MONTHS.	Wet and Stormy.	Fair.	Frost.	Snow & Hail.	East.	South-east.	South.	South-west.	West.	North-west.	North.	North-east.	Lowest.	Highest.	Mean.	Lowest.	Highest.	Mean.	Lowest.	Highest.	Mean.	At Catrine.	At Sorne Manse.
													Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.
January,	8	4	16	3	3	10	...	5	2	3	...	8	29.19	30.35	29.64	52	61	56	27	47	36	0.60	0.76
February,	10	5	6	7	...	7	...	5	7	6	...	3	28.76	30.09	29.82	53	63	57	30	53	42	3.22	3.91
March,	14	12	4	1	1	7	2	9	4	4	...	4	29.02	30.42	29.99	55	63	59	40	55	47	3.61	4.15
April,	11	18	1	...	3	7	1	4	1	4	...	10	28.92	30.10	29.5	58	63	60	43	58	53	1.59	1.95
May,	4	25	...	2	...	8	2	7	2	6	...	6	29.28	30.09	29.72	52	68	60	42	75	60	1.33	0.90
June,	11	19	...	...	...	5	3	8	1	13	...	...	29.29	30.02	29.74	63	69	65	58	73	64	2.67	2.98
July,	11	20	...	...	...	2	4	13	3	4	1	4	29.22	30.20	29.76	63	72	67	59	79	66	3.80	3.74
August,	12	19	...	...	2	6	2	11	3	5	...	2	29.39	30.14	29.76	62	73	67	59	77	67	2.91	2.49
September,	11	19	...	...	...	5	5	8	4	6	1	1	29.31	30.07	29.68	60	65	62	57	65	60	2.51	2.61
October,	17	14	...	...	...	9	10	12	...	...	...	...	29.03	30.18	29.45	58	67	63	51	63	57	6.49	6.70
November,	13	6	7	4	...	5	3	4	5	13	...	...	28.85	30.26	29.56	53	65	58	34	55	46	5.57	5.61
December,	17	9	5	...	...	9	2	7	3	3	...	7	28.38	30.38	29.45	55	53	59	33	59	44	3.51	4.18
	139	170	39	17	9	80	34	93	35	67	2	45	29.05	30.19	29.67	57	66	61	44	63	53	37.91	39.98
1830,	185	109	56	15	18	79	35	91	55	65	7	15	28.95	30.13	29.51	54	66	58	39	59	49	39.51	45.04
+		61		2		1		2		2		30	.10	.06	.16	3		3	5	4	4	1.60	5.06
—	45		17		9		1		20		5												

Description of several New or Rare Plants which have lately flowered in the neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany in the University of Edinburgh.

June 10. 1832.

Andromeda tetragona.

A. *tetragona;* foliis quadrifariam imbricatis, appressis, subtriquetris, obtusis, glabris (?); pedunculis elongatis, solitariis unifloris; corollis campanulatis.—*Spreng.*

Andromeda tetragona, *Linn.* Fl. Suecic. ed. 2. No. 356.—*Willd.* 2. 607. —*Wahlenb.* Fl. Lappon. No. 200.—*Pursh,* Fl. Amer. sept. 1. 290.— *Spreng.* 2. 289.

Andromeda pedunculis solitariis lateralibus, corollis campanulatis, foliis oppositis obtusis imbricatis revolutis.—*Gmel.* Fl. Siberic. 4. 120. No. 5.

DESCRIPTION.—*Stem* erect, woody (about 5 inches high) naked near the base, and marked by the origin of fallen leaves, much branched; branches suberect, the lower decumbent at the base and rooting. *Leaves* (two lines long) in four rows, closely imbricated, sagittate, concave in front, triquetrous and furrowed over the midrib behind, blunt, slightly pubescent, particularly in native specimens, but the degree seems to vary, as does the colour, which is bright or dull green. *Peduncles* axillary, solitary, at first short, afterwards much elongated, slightly pubescent, sheathed with scales at the base. *Flower* drooping. *Calyx* 5-parted, greenish, tipped with red, glabrous, persisting, segments gibbous at the base. *Corolla* white, campanulate, somewhat contracted near the mouth, which is 5-cleft, the segments blunt and spreading. *Stamens* included, filaments shorter than the pistil, erect. *Anthers* yellow, each with two slender spreading hispid bristles. *Pistil* scarcely longer than the stamens; stigma obtuse; style persisting, straight, slightly tapering upwards. *Germen* roundish-oval, obscurely 4-lobed, depressed at the insertion of the *style,* and surrounded at the base by a wrinkled glandular ring. *Capsule* erect, nearly globular, glabrous, having 5 loculaments, the dissepiments arising from the centre of the valves, which are inflected in their apices.

The seeds of this interesting little plant, which surely will yet be found indigenous in Britain, were kindly communicated to the Botanic Garden, Edinburgh, by Dr Richardson and Mr Drummond, on the return from North America of the last expedition under the command of Captain Franklin. It flowered, for the first time, in April 1832, in the same border with, but a little later than, its beautiful compatriot *Andromeda hypnoides.* We have two varieties: one only has yet, by flowering, rewarded the judicious cultivation of Mr Macnab. It is the lighter coloured, and considerably the freer growing of the two.

Arbutus pilosa.

A. *pilosa;* caule frutescente, procumbente, piloso: foliis ovato-ellipticis, ciliato-serrulatis, coriaceis, apice muticis, callosis; pedunculis axillaribus, unifloris, elongatis, nutantibus; antheris quadri aristatis.

DESCRIPTION.—*Stem* branching from the root, prostrate, red, twiggy, covered with thickset, harsh, spreading, rusty-coloured hairs. *Leaves* (9 lines long, 4½ broad) scattered, spreading, and being turned to the light, are distichous, coriaceous, naked and shining on both sides, dark green in front, pale behind, ovato-elliptical, with a callous tip, but no mucro, veined, serrulate, each serrature being tipped with a hair similar to those on the stem, a very few also occasionally exist on or near the middle rib behind. *Petioles* short, subappressed, and with rather tumid axillary buds. *Peduncles* sparingly covered with a few fulvous hairs, solitary in the axils of a few of the terminal leaves, of which they are equal to one-half the length. *Bracteæ* ovate, scattered upon the peduncle, adpressed, larger and fewer upwards. *Calyx* 5-cleft, persisting, white, glabrous within and without, spreading, segments ovate, acute, gibbous at the base. *Corolla* (3 lines long) ovate, white, 5-toothed, teeth blunt and revolute. *Stamens* 10, arising from a small green disk; filaments white, covered with minute pubescence, swollen immediately above their origin, and there somewhat concave on their inner surface, subulate upwards; anthers yellow, attached by their backs, ovato-oblong, each loculament with two small ascending awns, in front of which it opens by a pore. *Stigma* small, red, terminal, very obscurely 5-lobed. *Style* erect, cylindrical, included, colourless. *Germen* ovate, green, rather more than half the length of the style, and equal to the filaments, slightly covered with obscure pubescence, and depressed on the top, where the style is inserted.

This species is nearly allied to *A. mucronata*, which flowered in the Botanic Garden lately, and is figured in Bot. Mag. t. 3093., but is easily distinguished by the character given above. They undoubtedly belong to the same genus, but whether they should be left as species of *Arbutus*, or removed to *Gualtheria* or *Arctostaphylos*, or erected into a new genus, must be chiefly regulated by the fruit, which I have not seen. I doubt whether the calyx, though persisting, will become berried as in *Gualtheria*, but the anthers are, as in that genus, provided with 4 awns. The present species is a native of Mexico, and was raised by Mr Neill from seed received from Mr Don. From Mr Neill we received it at the Botanic Garden. In both establishments it flowered during May, and is perfectly hardy.

Epacris ceræflora.

E. *ceræflora;* ramulis tomentosis; foliis lanceolatis, acuminatis, patentissimis; floribus patulis, pedunculatis, secundis; calycibus acutis, ciliatis, tubo corollæ longe brevioribus.

Description.—*Stem* erect, branched. *Branches* tomentous, purplish. *Leaves* lanceolate, acuminate, dark green above, paler below, mucronate, subpetiolate, spreading wide. *Flowers* collected near the extremities of the branches, white, secund, peduncled, patent. *Calyx segments* lanceolate, ciliated. *Corolla*, tube obscurely pentagonal, thrice as long as the calyx, pitted on the outside between the calyx segments, and having nectariferous depressions under the corresponding elevations within, somewhat contracted upwards; limb revolute, segments subacute. *Stamens* subexserted; filaments alternating with the nectariferous pores, and adhering through their whole length to the inside of the corolla; anthers dark leaden-coloured, pollen granules white. *Stigma* capitate, sublobate, flattened on the top. *Style* glabrous, somewhat thickened above its base, and again contracted, tapering a little towards the stigma. *Germen* green, glabrous, subrotund. *Unripe capsule* subturbinate, pitted at the insertion of the style. *Seeds* erect, on a central placenta.

This species, a native of Van Diemen's Land, was raised at the Botanic Garden, Edinburgh, from seeds communicated by Mr Newbigging, and likewise by the Rev. Mr Craig, in January 1831. It flowered for the first time in April and May 1832, the plants being still very small. It appears to be ripening seed abundantly.

Francoa appendiculata.

F. *appendiculata;* caulescens, foliis lyratis, denticulatis utrinque pubescentibus, lobo terminali maximo cordato obtuse angulato; floribus spicatis.

Francoa appendiculata, *Cavan.* Icon. vi. 77. t. 596.—*Pers.* Synops. 1. 445. —*Sprengel,* Sp. Plant. 2. 262.

Francoa sonchifolia? *Ad. Juss.* Ann. des Sc. Nat. 3. 192. t. 12.

Description.—*Root* with several very leafy crowns. *Stems* short. *Leaves* (8 inches long) petioled, lyrate, with soft, slightly glutinous pubescence on both sides, bullate, undulate, strongly veined, denticulate, decurrent along the petiole; lobes blunt, the terminal one by much the largest (in a vigorous plant 6 inches long, 4½ inches broad), bluntly angled, cordate at the base. *Flower-stalk* (2 feet high) terminal, scape-like, having a few leaves at the base only, erect, straight, round, slightly tapering, densely covered with pubescence similar to that on the leaves; from the axils of the stem-leaves, and from a bractea near the top, arise solitary erect branches, in all respects similar to the primary shoot, but smaller. *Spike* (6 inches long) racemose, flowers (½ inch long, ¾ inch across when fully epxanded) rather dense, springing from the axils of lanceolato-lnear, green *bracteæ*. *Calyx* persisting, 4–5-parted, green, rather longer than the pedicel, segments ovato-acute, 3-nerved, glanduloso-pubescent within and without. *Petals* 4–5, twice the length of the calyx, obovato-elliptical, channelled in front towards the short claw, keeled behind, of a pale rose-colour, with a darker spot in the centre, becoming lighter after expansion. *Stamens* 8–10, shorter than the calyx, alternating upon an obscure but nectariferous disk, with short diverging scales (abortive stamens); filaments subulate, glabrous; anthers yellow, bilocular, oblong, bifid at both extremities, and slightly diverging at the lower, bursting along the sides, pollen yellow, granules small. *Germen* superior, oblong, 4–5-furrowed, 4–5-valved, and having as many loculaments, formed by the inversion of the margins of the valves. *Stigma* sessile, 4–5-lobed, at first involute, then spreading, peltate, fleshy, surface tubercled. *Ovules* numerous, green, oblong.

This showy plant was introduced into the Clapton nursery from Chiloe by Mr Anderson. From Clapton it was obtained by Mr Cunningham at Comely-bank, near Edinburgh, and communicated to Mr Neill's garden at Canonmills. In both these establishments it flowered in May 1832. I have no doubt of this being the species of Cavanilles, and very little about its being that of Jussieu, though the petals are figured (not described) by Cavanilles as acute, and though the flowers are said by Jussieu to be without pedicels in his plant. The leaves correspond with Cavanilles', and the station is the same. His figure represents the flowers as secund, and a dried specimen brought home by Mr Anderson, and given to Mr James Macnab, has the same appearance.

Kennedia Comptoniana.

K. *Comptoniana,* var *quinquefolia.*

This is a very handsome variety, but I can perceive no good character by which it can be specifically distinguished. It is very robust, the leaves being much longer and narrower than in the usual form of the species, and very often with 5 folioles, the terminal one only, as in the common variety, being supported upon an elongated petiole. The flowers are in a dense raceme, and of a deep rich lilac, with two green striated spots on the vexillum. It was raised from seeds sent to the Botanic Garden, Edinburgh, from New Holland, by Mr Fraser in 1827, and flowered in the greenhouse for the first time in April 1832.

Menziesia empetriformis.

M. *empetriformis;* foliis linearibus serrulatis; pedunculis aggregatis; floribus campanulatis, erectis, decandris; calycibus glabris, obtusis, basi gibbosis; antheris filamenta æquantibus.

Menziesia empetriformis, *Smith*, in Linn. Soc. Trans. 10. 380.—*Pursh*, Flor. Americ. Septent. 1. 264.—*Nuttall*, Genera, 1. 252.—*Sprengel*, Syst. Veget. ii. 202.

DESCRIPTION.—A small erect *shrub*. *Leaves* (6 lines long, 1 line broad) linear, on short adpressed petioles, crowded, suberect towards the extremities of the branches, below spreading, when young glanduloso-ciliated, afterwards glabrous, with a few cartilaginous small teeth especially towards the apices, slightly channelled above, fleshy in their sides, midrib somewhat depressed, flattened and wrinkled. *Peduncles* (½ inch long) erect, glandular, axillary, single and single-flowered, collected near the extremities of the branches, bibracteate at the base. *Bracteæ* ovate, concave, crenate, opposite. *Calyx* 5-phyllous, red without, green within, except on the edges where it is red, glabrous, ciliated with minute white hairs, leaflets blunt, wrinkled and gibbous at the base. *Corolla* (3 lines long, 2 broad) reddish-purple, campanulate, erect, glabrous, about three times as long as the calyx, 5-toothed, teeth reflected. *Stamens* 10, of rather unequal length alternately, about the length of the germen ; filaments rose-coloured, flat, linear ; anthers purple, oblong, narrower at the upper end, as long as the filaments, connivent, grooved along their sides, bursting by two terminal pores, attached by their backs to the filaments. *Pistil* exserted ; stigma of 5 connivent, triangular teeth ; style slightly curved, cylindrical, red ; germen globular, green, glandular, quinquelocular ; ovules very numerous, attached to a large central placenta.

This very distinct species of *Menziesia* was raised at the Botanic Garden, Edinburgh, from seeds communicated by Mr Drummond on his return from the last expedition to North America under the command of Captain Franklin, and, I believe, collected by him (Mr Drummond) on the Rocky Mountains. It first flowered in November 1831, but much more abundantly in May 1832.

If Sir James Smith had seen the living plant, I think he would have given a different specific character. The leaves, in the recent state, are decidedly tumid, both above and below, being depressed only along the middle rib on either side.

Pimelea sylvestris.

P. *sylvestris ;* foliis oppositis utrinque glabris, lanceolatis, acutis ; floralibus 4–5, rameis subsimilibus, capitulo terminali multifloro brevioribus ; perianthiis glabris, tubo infundibuliformi.—*Br.*

Pimelea sylvestris, *Br.* Prodr. Fl. Nov. Holland. 361.—*Ræm. et Schultes*, Syst. Veget. 1. 274.—*Spreng.* Syst. Veget. 1. 92.

DESCRIPTION.—*Stem* erect, shrubby, twig-like, bark covered with minute warts. *Leaves* (above 1 inch long, ¼th of an inch broad) opposite, in five rows, on short adpressed petioles, spreading, glabrous on both sides, lanceolate, acute, quite entire, flat above, or slightly turned back at the edges, very obscurely veined, middle rib channelled below ; *floral leaves* similar to those of the branches. *Capitulum* terminal, many-flowered, flowers expanding from without towards the centre, much longer than the floral leaves. *Perianth* (above half an inch long) glabrous, funnel-shaped, pale rose-coloured, becoming white after expansion, throat naked ; limb 4-parted, segments erect, ovate, their edges folded outwards at their base, the two outer segments in the bud subacuminate, and keeled at the apex, the two inner blunt; tube much attenuated downwards, but dilated at its base where it covers the germen, and is persistent, all above being deciduous. *Filaments* adhering to the throat, above this free, longer than the perianth, reflected. *Anthers* orange-yellow, linear. *Stigma* capitate, small. *Style* erect, filiform, exserted, glabrous, colourless. *Germen* ovate, compressed, glabrous. *Ovule* single, pendulous.

The seeds of this species were collected on the south coast of New Holland in July 1829, and being kindly sent by Colonel Lindesay, were received at the Botanic Garden, Edinburgh, twelve months afterwards. Several plants flowered freely in the greenhouse in May 1832. They form a handsome addition to the species previously in cultivation. There is reason to believe that the seeds will ripen.

Rulingia corylifolia.

R. *corylifolia;* foliis ovato-deltoideis, subcordatis, basi lobatis supra hispidis, subtus hirsuto-tomentosis; stipulis ovato acuminatis; corymbis oppositifoliis; filamentis antheriferis simplicibus, sterilibus ovato-lanceolatis alternantibus.

DESCRIPTION.—*Shrub* branched from the base of the stem, branches slightly flexuose, tomentoso-villous, and slightly viscid. *Leaves* (2½ inches long, 2 inches broad) ovato-deltoid, slightly cordate, slightly lobed at the base, serrato-crenate, rugose, pubescent on both sides, but much more considerably behind, where also they are paler, bright green above, and, when fading, becoming red, being very prominent behind; petioles slightly channelled above, villous, much shorter than the leaves, bistipulate. *Stipulæ* opposite, distinct from the petiole, ovate, acuminate, villous, and with long ciliæ. *Corymbs* collected near the apices of the branches, densely covered with white hairs in its primary and subsequent divisions, each division having on the outside a lanceolate bractea. *Flowers* pedicellate, white. *Calyx* 5-phyllous; phylla cordate, villous both within and without, but much more harshly without, somewhat reflected in their sides, and forming a prominent edge where they meet each other. *Petals* pubescent, much smaller than the calyx segments, concave, gibbous at their base, their sides formed into two blunt parallel wings, which project towards the axis of the flower, apex extended into a blunt linear appendage, at first curved towards the axis, but afterwards bent back, and passing out between the segments of the calyx. *Stamens* 5 (perfect), immediately within the petals, and alternating with the segments of the calyx, shorter than the petals, and included within their folds, alternating on the same urceolate border with, and somewhat shorter than, ovato-lanceolate scales (abortive stamens), which are hairy on the outside, smooth within; filaments glabrous; anthers short, bilocular, bursting along the sides. *Pollen* yellow, granules round. *Stigmata* cohering to each other, small, capitate, colourless, shining. *Styles* 5, glabrous, in contact in the centre of the flower, scarcely longer than the stamens. *Germen* 5-lobed; in its early stages lobes conical and a little rough, afterwards rounded, green, depressed in the centre, and densely covered with stellate pubescence, 5-locular, dissepiments from the edges of the valves, their two layers afterwards separating. *Ovules* two in each loculament, with a central ridge of the valve between them, both attached to the central column below its middle. Pubescence every where on the plant stellate, except from abortion, when, as on the upper surface of the leaves, it often appears single.

This plant was received last year by Mr Neill at Canonmills, and in the Botanic Garden, Edinburgh, from Mr Knight on the King's Road; and both with Mr Neill and us, it flowered freely in the greenhouse in May last. I am indebted to my acute friend Dr Hooker for many important references regarding this genus and its allies, which led me to investigate its structure much more closely than I otherwise would, and prevented my committing some mistakes.

Stylidium hirsutum.

S. *hirsutum;* scapo hirsuto villis acutis; racemo subsimplici; calycis labia (⅔-) partita; capsula ventricosa ovata; foliis linearibus, basi attenuatis, margine parum recurvis, squamis scariosis distinguantibus interioribusque acuminatis.—*Br.*

Stylidium hirsutum, *Br.* Prodr. Flor. Nov. Holland. 568.—*Spreng.* Sp. Plant. 3. 747.

Description.—*Root* of strong, hard, branching fibres. *Leaves* (6 inches long) all radical, linear, glabrous, firm in their texture, edges revolute, attenuated at the base, interspersed with scariose glabrous scales, which become larger towards the innermost, these being terminated with a point resembling the leaves, but shorter. *Scape* (9 inches high) erect, simple, rather longer than the leaves, covered, especially at the base, with long spreading colourless acute (not glandular) hairs, smoother upwards. *Raceme* (1½ inch long) spicate, the uppermost flowers expanding first, each rising from the axil of a lanceolate green bractea, which is covered with hairs similar to those on the scape. *Pedicels* hairy, half the length of the primary bracteæ, and having secondary lateral bracteæ. *Calyx* ⅔-partite; tube very hairy, having both pointed hairs and others which are shorter and glandular; segments connivent, blunt, having glandular hairs only, the two outer the largest and broadest. *Corolla* purplish-rose coloured, yellow in the throat, as well as the calyx covered on the outside with glandular pubescence, the four larger segments nearly equal, spreading, flat, channelled in the centre, and slightly crisped on the edges, the two next the labellum rather the narrowest, and having each one erect, ovate, entire tooth at its base, of similar colour with the rest of the corolla, the two others green at their base on the outside, and furrowed in the throat, the groove having prominent, erect, pubescent edges; labellum deflected from the inside of the calyx between the lips, small, ovate, acute, yellow, with a purple crisped and crenate edge, its appendices blunt spreading and much shorter than itself; tube pale yellow, twisted, equal to the longest segments of the calyx, the whole of the inside and the upper surface of the limb presenting, under the microscope, a beautiful crystalline appearance. *Column* linear, flat, equal in length to the limb, dark red in front, yellow behind, glabrous, very irritable, bordered at its lower part. *Anthers* leaden-coloured, pollen granules lilac, minute, ovate. *Stigma* of dull green colour, oblong, glandular surface crystalline. *Germen* ovate, bilocular, dissepiment imperfect above. *Ovules* very numerous, attached to a central receptacle, awanting in the lower part of the dissepiment.

This species is new in cultivation, and the flowers are larger than any in our gardens. I owe to the late Mr Fraser, colonial botanist, a native specimen collected at King George's Sound, on the south coast of New Holland; and, from seed picked off one sent at the same time to Mr Macnab, the plant described was raised. It flowered in the greenhouse of the Royal Botanic Garden in May, and will continue to bear flowers during the early part of June.

Symphitum Caucasicum.

S. *Caucasicum;* caule ramoso, inferne hirsuto, superne glutinoso; foliis ovato-lanceolatis, base attenuatis, semidecurrentibus, hirsutis; calycibus obtusis.

Symphitum Caucasicum, *Marsch. Bieb.* Fl. Tauric. Caucas. 1. 128.—*Sprengel,* System. Veget. 1. 563.

Description.—*Stem* (2 feet high) hirsute near the bottom, higher up pubescent and viscous, slightly winged, flexuose, branched. *Leaves* ovato-lanceolate, hirsute on both sides, but less harshly on the upper, and there when young subviscid, half decurrent, the lower attenuated at the base, the upper pair oblique, sessile and alternate. *Spikes* terminal, geminate, many-flowered, secund and involute, common peduncle and pedicels glanduloso-pubescent. *Calyx* angled, the angles and blunt teeth ciliated, when in fruit distichous. *Corolla* at first red-purple, but loses this colour as soon as it expands, and acquires a lively azure hue; tube longer than the calyx, sparingly and minutely pubescent on the outside, having a white fleshy narrow edge projecting internally from its base over the disk; teeth of the limb blunt and revolute in their edges; teeth of the throat erect, blunt, and having short crystalline ciliæ on their edges. *Stamens* included, about as long as the teeth; filaments purplish; anthers yellow, rather shorter than the free portion of the filaments, bifid at both extremities. *Pistil* rather longer than the stamens; stigma bilobular, rounded; style slightly tapered, glabrous, lilac; germen light yellowish-green, seated on a white disk. The unripe achenia are rough, irregularly depressed over their surface, and each is raised on a sand-glass shaped portion of the disk, the upper lobe of which projects from its lower side a simple row of short dependent subulate hairs.

The seeds of this plant were received at the Royal Botanic Garden, from Dr Fischer, under the name adopted, in 1830, and flowered for the first time in May 1832. The profusion of lively coloured flowers in this species, which is less deformed by coarseness of herbage than others, makes it one of the most desirable for cultivation.

Tropæolum tricolorum.

T. *tricolorum;* caule tenuissimo scandente ramoso, foliis peltatisectis; segmentis 6–7 oblongis obovatisve integris basi attenuatis, petiolis cirrhosis, petalis unguiculatis calyce persistente subclauso parum longioribus obtusis integerrimis.—*Sweet.*

Tropæolum tricolorum, *Sweet's* British Flower Garden, 270.

Description.—*Root* tuberous. *Stem* filiform, greatly branched, branches entangled, purple, shining. *Leaves* alternate, petioled, palmato-digitate, round, (8 lines across) 6-lobed, soft, slightly villous, especially below where paler, veined, lobes unequal, obovato-elliptical, generally only one of them mucronate; petiole (1 inch long) filiform, resembling the branches. *Peduncles* (above 2 inches long) solitary, opposite to the leaves, pendent, capillary, slightly thickened upwards. *Calyx* of bright vermilion colour, pentagonal, 5-cleft, the segments blunt, mucronulate, on the outside tipped as well as the spur with purple, on the inside tipped with green, whole inner surface glandular; spur erect, about one-third of the length of the peduncle, awl-shaped, nectariferous. *Petals* 5 (3 lines long), yellow, subexserted, inserted below the incisures of the calyx, obcordato-spathulate, unguiculate, dilated at the base over a slightly villous pit. *Stamens* 8, included; filaments glabrous, colourless, dilated at the base, and having on the outside of the insertion of each a pit similar to that at the base of the petals; anthers yellow, cernuous. *Germen* glabrous, 3-lobed, lobes keeled. *Style* glabrous, shorter than the stamens, grooved on three sides, 3-toothed at the top, one of the teeth larger than the others, and grooved.

We received this plant from Mr Anderson of the Apothecaries' Garden, Chelsea. It produced its truly splendid flowers in the greenhouse of the Botanic Garden in March and April.

Celestial Phenomena from July 1. to October 1. 1832, calculated for the Meridian of Edinburgh, Mean Time. By Mr George Innes, Astronomical Calculator, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight —The Conjunctions of the Moon with the Stars are given in *Right Ascension*.

JULY.

D.	H.	′	″		D.	H.	′	″	
1.	15	38	–	☌ ☿ ι ♊	20.	21	55	43	☾ Last Quarter.
1.	22	0	23	☌ ♀ μ ♊	21.	4	35	7	☌ ☽ ♂
2.	1	34	40	Im. II. sat. ♃	21.	10	56	30	☌ ☽ 2 ξ Ceti.
2.	13	38	43	☌ ☽ ♄	21.	18	40	13	☌ ☽ μ Ceti.
3.	8	2	–	Sup. ☌ ☉ ☿	22.	15	13	1	☌ ☽ f ♉
4.	23	24	31	☽ First Quarter.	22.	22	15	21	☉ enters ♌
7.	1	58	4	Im. I. sat. ♃	23.	0	14	18	Im. I. sat. ♃
8.	2	41	16	☌ ☽ γ ♎	23.	11	54	35	☌ ☽ γ ♉
8.	3	7	–	☌ ☿ κ ♊	23.	13	9	15	☌ ☽ 1 δ ♉
9.	5	56	31	☌ ☽ φ Oph.	23.	13	38	2	☌ ☽ 2 δ ♉
9.	8	14	22	☌ ♂ ο ♓	23.	18	29	39	☌ ☽ α ♉
11.	0	16	30	Im. IV. sat. ♃	24.	18	43	22	☌ ☽ ζ ♉
11.	6	59	15	☌ ☽ 1 μ ♐	25.	14	10	34	☌ ☽ η ♊
11.	7	41	2	☌ ☽ 2 μ ♐	26.	1	32	19	Em. III. sat. ♃
12.	7	3	53	☌ ☽ ο ♐	26.	3	14	11	☌ ☽ ζ ♊
12.	9	30	38	☌ ☽ π ♐	26.	22	42	9	Im. II. sat. ♃
12.	15	1	10	☌ ♀ δ ♊	27.	9	24	36	Sup. ☌ ☉ ♀
12.	22	47	55	○ Full Moon.	27.	13	49	1	● New Moon.
14.	17	23	35	☌ ☽ ϑ ♑	27.	14	19	18	☌ ☽ ♀
15.	5	15	40	☌ ☽ ♅	27.	21	44	–	☌ ☿ α ♌
15.	10	14	19	☌ ☽ γ ♑	28.	22	8	36	☌ ♀ δ ♋
16.	13	42	4	☌ ☽ δ ♑	29.	4	32	–	☌ ☽ ☿
18.	12	32	54	☌ ☽ ♃	30.	2	8	20	Im. I. sat. ♃
20.	12	36	39	☌ ☽ ι ♓	30.	3	20	8	☌ ☽ ♄

AUGUST.

D.	H.	′	″		D.	H.	′	″	
1.	11	48	–	☌ ☿ ρ ♌	10.	23	29	5	☌ ☽ ϑ ♑
2.	2	21	9	Im. III. sat. ♃	11.	9	31	41	☌ ☽ ♅
3.	1	18	28	Im. II. sat. ♃	11.	11	5	–	☌ ☿ ♄
3.	10	42	25	☽ First Quarter.	11.	14	19	43	○ Full Moon.
4.	8	57	37	☌ ☽ γ ♎	11.	16	15	14	☌ ☽ γ ♑
5.	12	3	13	☌ ☽ φ Oph.	11.	19	42	0	☌ ☽ δ ♑
7.	13	6	11	☌ ☽ 1 μ ♐	12.	16	2	–	☿ greatest elong.
7.	13	48	22	☌ ☽ 2 μ ♐	14.	16	5	11	☌ ☽ ♃
7.	22	30	57	Im. I. sat. ♃	14.	22	22	16	☌ ♀ α ♌
8.	13	13	18	☌ ☽ ο ♐	15.	0	25	41	Im. I. sat. ♃
8.	15	40	14	☌ ☽ π ♐	16.	18	29	55	☌ ☽ ι ♓
9.	4	46	–	☍ ☉ ♅	17.	17	10	19	☌ ☽ 2 ξ Ceti.

AUGUST—*continued.*

D.	H.	′	″	
18.	1	3	28	☌ ☽ μ Ceti.
18.	17	34	48	☌ ☽ ♂
18.	22	6	25	☌ ☽ f ♉
19.	6	23	7	☾ Last Quarter.
19.	17	4	–	☌ ☿ υ ♌
19.	19	24	54	☌ ☽ γ ♉
19.	20	41	59	☌ ☽ 1 δ ♉
19.	21	11	42	☌ ☽ 2 δ ♉
20.	2	12	44	☌ ☽ α ♉
20.	3	14	12	☌ ♀ ρ ♌
21.	3	15	14	☌ ☽ ζ ♉
21.	23	20	52	☌ ☽ ν ♊
22.	2	19	37	Im. I. sat. ♃
22.	12	48	5	☌ ☽ ζ ♊
23.	4	43	45	☉ enters ♍
23.	20	48	17	Im. I. sat. ♃
24.	3	11	25	☌ ☽ δ ♋
25.	21	32	55	● New Moon.
26.	10	41	53	☌ ☽ ♀
26.	18	58	6	☌ ☽ ♄
27.	4	9	–	☌ ☽ ☿
27.	22	25	0	Im. II. sat. ♃
30.	22	42	52	Im. I. sat. ♃
31.	2	48	44	☌ ♀ ♄
31.	16	39	19	☌ ☽ γ ♎
31.	18	45	–	☌ ☿ υ ♌

SEPTEMBER.

D.	H.	′	″	
1.	19	17	15	☌ ☽ φ Oph.
2.	1	17	31	☽ First Quarter.
2.	8	40	–	☌ ☿ ♀
3.	19	54	55	☌ ☽ 1 μ ♐
3.	20	36	59	☌ ☽ 2 μ ♐
4.	1	0	59	Im. II. sat. ♃
4.	19	59	4	☌ ☽ ο ♐
4.	22	25	52	☌ ☽ π ♐
5.	4	15	–	☌ ☿ ♄
6.	0	13	17	☌ ♀ β ♍
6.	22	27	55	Im. III. sat. ♃
7.	0	37	36	Im. I. sat. ♃
7.	6	14	19	☌ ☽ ϑ ♑
7.	14	7	27	☌ ☽ ♅
7.	22	58	17	☌ ☽ γ ♑
8.	2	24	24	☌ ☽ δ ♑
9.	2	48	–	Inf. ☌ ☉ ☿
10.	5	17	25	○ Full Moon.
10.	16	38	33	☌ ☽ ♃
11.	3	13	25	☌ ☉ ♄
11.	3	36	57	Im. II. sat. ♃
12.	11	5	26	☌ ♀ η ♍
13.	0	10	2	☌ ☽ ν ♓
13.	22	42	37	☌ ☽ 2 ξ Ceti.
14.	2	29	41	Im. III. sat. ♃
14.	2	32	30	Im. I. sat. ♃
14.	6	34	46	☌ ☽ μ Ceti.
15.	3	40	15	☌ ☽ f ♉
15.	21	1	13	Im. I. sat. ♃
15.	21	43	9	☌ ☽ ♂
16.	1	28	17	Im. IV. sat. ♃
16.	2	28	25	☌ ☽ 1 δ ♉
16.	2	58	31	☌ ☽ 2 δ ♉
16.	13	17	37	☍ ☉ ♃
17.	9	35	45	☌ ☽ ζ ♉
17.	13	26	37	☾ Last Quarter.
18.	4	33	–	☍ ☉ ⚴
18.	6	12	57	☌ ☽ ν ♊
18.	20	4	24	☌ ☽ ζ ♊
19.	2	19	40	☌ ☽ δ ♊
20.	11	40	27	☌ ☽ δ ♋
21.	22	12	59	Em. II. sat. ♃
22.	21	22	–	☌ ☽ ☿
23.	1	9	2	Em. I. sat. ♃
23.	1	14	8	☉ enters ♎
23.	10	31	17	☌ ☽ ♄
23.	13	56	9	☌ ♀ ϑ ♍
24.	6	47	19	● New Moon.
24.	19	37	52	Em. I. sat. ♃
24.	20	32	–	☿ greatest elong.
25.	9	28	46	☌ ☽ ♀
27.	3	22	24	☌ ♂ σ ♌
28.	1	38	18	☌ ☽ γ ♎
28.	10	11	51	☌ ☽ ϑ ♎
28.	0	48	29	Em. II. sat. ♃
29.	3	45	4	☌ ☽ φ Oph.
30.	3	4	15	Em. I. sat. ♃
30.	7	3	–	☿ near ♄
31.	18	15	28	☌ ♂ ε ♉

Times of the Planets passing the Meridian, and their Declination.

JULY.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	11 54	24 23 N.	11 31	23 37 N.	6 38	5 43 N.	5 18	1 55 S.	16 12	9 27 N.	2 46	16 8 S.
5	12 16	24 1	11 38	23 36	6 32	6 43	5 3	1 51	15 58	9 18	2 29	16 11
10	12 42	22 40	11 48	23 18	6 26	7 56	4 44	1 48	15 40	9 7	2 9	16 13
15	13 3	20 27	11 51	22 43	6 19	9 7	4 26	1 48	15 23	8 56	1 48	16 16
20	13 20	17 41	11 59	21 52	6 11	10 14	4 6	1 49	15 4	8 44	1 29	16 20
25	13 32	14 37	12 6	20 44	6 3	11 18	3 46	1 52	14 46	8 32	1 8	16 24

AUGUST.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	13 43	10 6 N.	12 13	18 44 N.	5 52	12 42 N.	3 17	1 58 S.	14 22	8 14 N.	0 40	16 29 S.
5	13 46	7 37	12 17	17 25	5 44	13 27	3 0	2 4	14 8	8 4	0 24	16 32
10	13 47	4 35	12 22	15 34	5 38	14 20	2 40	2 13	13 50	7 50	23 58	16 37
15	13 43	1 57 N.	12 26	13 32	5 30	15 10	2 19	2 24	13 32	7 36	23 38	16 40
20	13 34	0 9 S.	12 30	11 22	5 20	15 55	1 57	2 36	13 15	7 22	23 18	16 43
25	13 19	1 30	12 34	9 3	5 10	16 37	2 36	2 50	12 58	7 8	22 57	16 46

SEPTEMBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	12 45	1 16 S.	12 39	5 40 N.	4 57	17 30 N.	1 6	3 10 S.	12 33	6 47 N.	22 29	16 51 S.
5	12 18	0 10 N.	12 41	3 40	4 48	17 57	0 48	3 29	12 20	6 36	22 13	16 53
10	11 45	2 27	12 44	1 7 N.	4 37	18 28	0 26	3 35	12 3	6 21	21 52	16 56
15	11 9	5 22	12 47	1 27 S.	4 25	18 56	0 4	3 53	11 42	6 7	21 32	16 58
20	10 53	7 18	12 50	4 1	4 11	19 20	23 38	4 11	11 27	5 52	21 12	17 0
25	10 49	7 16	12 53	6 33	3 57	19 42	23 16	4 26	11 10	5 38	20 52	17 3

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *First View of Sierra Leone.*—Dr Boyle, in his interesting work on the medical topography of the western coast of Africa, says, "There are very few parts in the tropical world which at first sight hold out more allurements, even to the experienced traveller, than Sierra Leone. Its splendid scenery, and its beautiful river, together with its extensive, commodious, and generally secure harbour, and pleasant-looking town and villages, are calculated to excite the most flattering hopes in respect of health and enjoyment, notwithstanding strong previous impressions with regard to the contrary. On making Sierra Leone from the north, the mountains from which the peninsula was named first excite attention. They are lofty, perpetually clothed, from their summits to their bases, in all the fertile gaity of Nature's verdant scenery; and there is a pleasing and endless variety in the outline of their countless peaks and declivities. As the ship draws in with the shore, signs of cultivation appear, and increase with rapidity, both in number and attractiveness. Freetown, and the lately formed villages in its neighbourhood, at first appear like anomalous patches in the view; but on a nearer approach, they add greatly to its beauty and its interest. When the ship has arrived just at that point of distance from which a person may see all the broad outlines and apparent characteristics of an extensive scene, without being able to discern the minute details, the effect is magnificent. On the left hand is the Bulloon shore, low, but covered with luxurious and richly coloured bush, an occasional palm and pullom tree, rising in graceful form above the neighbouring mangroves:—in appearance it seems to embody the notions formed of fairy-land, but its realities most sadly illustrate the folly of such dreams. The middle ground also occurs on the left hand, and it gives a variety to the view. In front are the spacious river, extending farther than the eye can reach, and the north side of the peninsula, with its lofty mountains, and Freetown, running to the wa-

ter's edge, and surmounted by the barracks, and protected by a handsome fort, and a coast, forming small and convenient bays, from the town to its termination at the Cape, which runs boldly into the sea. On the right is the Atlantic. That a scene, composed of such ostensible material features, is grand and imposing, may readily be supposed; but those who are ignorant of the peculiarities of a tropical climate, and its seductive influence on a stranger, can form no adequate notion of the character and extent of its actual power. For the moment home is forgotten; or if remembered, the remembrance is accompanied with a desire it should be situated in such a seeming paradise. In thus speaking of the view on arriving at Sierra Leone, we are supposing the settlement to be made on a fine clear day, when the atmosphere is bright and comparatively devoid of malaria, and the river runs its natural course, unswollen, and free from discoloration. Should the arrival, however, happen at a different period, when the atmosphere is dense, oppressive, and fraught with deleterious exhalations, and the rains are deluging the face of the country, and at once augmenting the river, and destroying its beauty, then Sierra Leone presents a very different appearance; there is nothing to excite a pleasing anticipation, but there is a world of causes for apprehension and for dread. The realities of the scene are, of course, unaltered, for the two periods are the property of the climate, and must be alike endured by the colonists; but the appearances present a melancholy and fearful contrast."

2. *Description of an African Tornado.*—The seasons at Sierra Leone are divided into the wet and the dry. The latter is generally ushered in by the explosion of two or three tornados, which, although formidable in themselves, are still so long connected with the approach of a pleasant time, as that the inhabitants have sometimes prayed for their appearance. One of those strange commotions of nature is thus described by Mr Boyle:—" A violent tornado appears to strangers a most appaling visitation, and produces an extraordinary effect upon their feelings. It consists of successive flashes of the most vivid lightning, tremendous shocks of thunder, rapidly and alarmingly reiterated, impetuous gusts of wind, deluging rain. This terrific combination of the elements sweeps along the whole of

the coast under consideration; but it occurs with peculiar force on what is called the windward coast, especially at Sierra Leone. Its denomination is derived from the Portugese, it being a corruption of the word trueno, which means thunder-storm. Its approach is first discernible by the appearance of a small clear silvery speck, at a high altitude in the heavenly expanse, which increases and descends towards the horizon, with a gradual and slow, but visible motion. In its descent it becomes circumscribed by a dark ring, which extends itself on every side, and as soon as the silvery cloud approaches the horizon, veils it in impenetrable gloom. At the moment the elements seem to have ceased their operations, and the very functions of nature to be paralysed; the atmosphere appears to be deprived of the spirit of vitality, and a sensation of approaching suffocation pervades and oppresses the physical system. The mind is wrapped in awe and suspense, but the latter is speedily relieved by the dark horizon being suddenly illuminated by one broad blaze of electric fluid; peals of distant thunder then break upon the ear, and rapidly approach, and increase in fervency and violence, till the shocks become appaling; when the thunder is at its loudest, a tremendous gust of wind rushes with incredible and often irresistible vehemence from the darkened part of the horizon, not rarely in its course carrying away roofs of houses and chimney-tops, blowing down or uprooting trees, and laying the stoutest and largest ships on their beam-ends, or sinking them under weigh or at anchor; and to that succeeds a furious deluge of rain, which falls in one vast sheet, rather than in drops, and concludes this terrible convulsion. The lightning is of the most vivid description, and, contrary to what has been reported of it, seldom sheet-lightning, but forked and piercing, and often extremely destructive, both to things animate and inanimate. Its apparently doubtful, wild course, is sometimes directed to a large and lofty tree, and the foliage, at the points of contact, is blasted on the instant, the exposed branches are severed from the trunk, and probably the enormous trunk itself is rent to its basis and destroyed. When it comes in contact with a house, it frequently leaves it as great a wreck as ships have been seen to be on coming out of a severe action, or after a destructive storm; and, occasionally, the building entered by it may happen to remain untouched, and its inmates, some, or all of them, as the author has known to occur, perish under its scorching influence. Occasionally the spindle of a ship's mast, the most elevated part of it, may appear to be the point of attraction, and it will sometimes dart among the spars and cordage harmless, descending till it reach the deck, when it suddenly quits the vessel by some aperture, and rapidly returning through another, seems to have acquired a new character with incredible velocity; for, steering its strange and rapid course into the maindeck or hold, it will kill, maim, or injure every animate or inanimate with which it comes in contact. Much good has unquestionably been effected by conductors; but those who have watched the progress of the electric fluid, will hold the theorist in no estimation, who does not make the atmosphere the first and most important point of consideration. The heavy peals, or rather the terrifying shocks, of thunder which follow the lightning, frequently not only shake the buildings at Freetown, but the very foundations on which they stand; and the reverberations from the surrounding mountains increase, if possible, the awe excited by elementary commotion. The succeeding rain, or rather deluge, is happily of short duration, and rushing down the various inlets and indentations in the adjoining mountains, it forms into streams even a few minutes after its commencement, which sweeps through the streets of Freetown with astonishing velocity, bearing with them all the exposed vegetable and other matter, in a state of putridity or decay. Such is the tornado, and it is by the preponderating power of its gusts, and the atmospheric influence of lightning and its rains, that noxious exhalations from the earth, and deleterious miasmata, before confined to the neighbourhood of their origin by opposed or light currents of air in the day, or attracted by the land (the more lofty the more attractive) in the night, are removed, and consequently, the indescribably distressing feelings occasioned by a foul atmosphere, are superseded by those comparatively pleasurable and enlivening sensations which have been already noticed, pp. 40–42. The average time for the tornados to set in, is the termination of the month of September, from which time until Christmas, tolerably calm weather may be expected At Christmas, the periodical winds called the harmatan commence, and continue to blow for six or ten weeks. It is very curious, that whilst, to the natives, and to the Europeans, who, from long residence, may be said to be acclimated in the settlement, these winds are exceedingly annoying, the Europeans newly arrived consider them as refreshing and salubrious. But during the raging of the harmatans, the furniture of every house is covered with fine sand, and tables and chairs crack under their influence. Mr Boyle concludes this part of his subject by a diary of the weather at Sierra Leone, for the term nearly of a year,—a document that will be read with extreme interest by all the cultivators of meteorological science.

3. *On the distance to which Spray of the Sea may be carried.*—A few remarks on the distance to which spray from the sea is sometimes carried inland by storms of wind, may not, perhaps, be deemed altogether irrelevant to the subject we are treating upon. Sea-water is brought into the immediate neighbourhood of Manchester, which is at least thirty miles from the nearest coast, by every violent and long continued gale from the west; and the exact proportion in given quantities of rain-water, collected on several occasions of this kind, has been determined chemically. That the sea is the principal source whence the salt is derived, with which the rain that falls in this town and its vicinity is occasionally impregnated, cannot, I think, be doubted; as I have clearly ascertained, by direct experiment, that its excess or deficiency depends entirely on the direction, force, and duration of the wind. Rain collected in clean glass-vessels, a few miles to the north of Manchester, when the wind blows moderately from the north or north-east, scarcely ever exhibits the slightest trace of muriatic acid, on the application of the most delicate test (nitrate of silver), even when reduced two-thirds or three-fourths by spontaneous evaporation; though samples collected in the town, precisely at the same time, on being subjected to the test, generally have their transparency more or less impaired. This fact seems to prove, that, notwithstanding muriate of soda is never raised into the atmosphere by evaporation, yet the air over large towns usually contains a very minute portion of muriatic acid, which, as Mr Dalton observes, is probably supplied by the sublimation of muriate of ammonia during the combustion of fuel. A considerable increase of mu-

riatic acid takes place in the rain which falls in Manchester, when accompanied with a brisk breeze from the west, of several hours' duration; as is evident from the greater degree of opacity observed in samples caught under such circumstances, when treated with a few drops of the solution of nitrate of silver; and that which falls in the adjacent country, then manifests a sensible trace also. Indeed, the direction of the wind remaining the same, its force and duration seem almost entirely to regulate the quantity of muriatic acid in the atmosphere; which completely establishes the fact that it is brought from the sea by the mechanical action of powerful currents of air. The utmost distance to which sea-water is conveyed by tempestuous winds is not easily determined. Sir H. Davy, in his Elements of Agricultural Chemistry, p. 295, states, that " in great storms the spray of the sea has been carried more than fifty miles from the shore;" but he does not give his authority. Being at Blackwall, in Derbyshire, the residence of my relative John Blackwall, Esq. on the 23d of November 1814, when a violent hurricane occurred, which did extensive damage on the southern coast, I took several opportunities of examining the rain which fell at intervals on that occasion, and uniformly found that it became extremely turbid on application of the test, evidently containing much more muriatic acid than rain collected in large towns, during calm weather, is ever found to contain. The storm commenced on the night of the 22d of November, and continued, with little abatement, till after noon on the 23d. The wind blew from the south all the time, and the place of observation is 140 or 150 miles from the sea in that direction. This is, perhaps, the greatest distance on record to which sea-water has been clearly ascertained to be conveyed by the wind; and that it extended much further is highly probable.—*Manchester Memoirs*, vol. v. New Series.

ZOOLOGY.

4. *Wild Animals in the Illinois Country, in North America.* —The *buffalo* has entirely left us. Before the country was settled, immense prairies afforded pasturage to large herds of this animal, and the traces of them are still remaining in the " buffalo paths," which are to be seen in several parts of the

State. These are well beaten tracts, leading generally from the prairies in the interior of the state, to the margins of the large rivers; shewing the course of their migrations as they changed their pastures periodically, from the low marshy alluvium to the dry upland plains. In the heat of summer they would be driven from the latter by the prairie flies; in the autumn they would be expelled from the former by the mosquitoes; in the spring the grass of the plains would afford abundant pasturage, while the herds could enjoy the warmth of the sun, and snuff the breeze that sweeps so freely over them; in the winter, the rich cane on the river banks, which is an evergreen, would furnish food,—while the low grounds, thickly covered with brush and forest, would afford protection from the bleak winds. I know few subjects more interesting than the migration of wild animals, connecting, as it does, the singular display of brute instinct, with a wonderful exhibition of the various supplies which nature has provided for the support of animal life, under an endless variety of circumstances. These paths are narrow, and remarkably direct, shewing that the animals travelled in single file through the woods, and pursued the most direct course to their places of destination.——*Deer* are more abundant than at the first settlement of the country. They increase to a certain extent, with the population. The reason of this appears to be, that they find protection in the neighbourhood of man, from the beasts of prey that assail them in the wilderness, and from whose attacks, their young particularly can with difficulty escape. They suffer most from the wolves, who hunt in packs like hounds, and who seldom give up the chase until a deer is taken. We have often sat, on a moonlight summer night, at the door of a log-cabin on one of our prairies, and heard the wolves in full chace of a deer, yelling very nearly in the same manner as a pack of hounds. Sometimes the cry would be heard at a great distance over the plain; then it would die away, and again be distinguished at a nearer point, and in another direction;—now the full cry would burst upon us from a neighbouring thicket, and we would almost hear the sobs of the exhausted deer;—and again it would be born away, and lost in the distance. We have passed nearly whole nights in listening to such sounds; and once we saw a deer dash through the yard, and immediately pass the door at which we sat, followed by his audacious pursuers, who were but a few yards in his rear.—Immense numbers of deer are killed every year by our hunters, who take them for their hams and skins alone, throwing away the rest of the carcass. Venison hams and hides are important articles of export: the former are purchased from the hunters at 25 cents a pair, the latter at 20 cents a pound. In our villages we purchase for our tables the saddle of venison, with the hams attached, for 37½ cents, which would be something like 1 cent a pound.—There are several ways of hunting deer, all of which are equally simple. Most generally the hunter proceeds to the woods on horseback, in the day-time, selecting particularly certain hours, which are thought to be most favourable. It is said, that, during the season when the pastures are green, this animal rises from his lair precisely at the rising of the moon, whether in the day or night; and I suppose the fact to be so, because such is the testimony of experienced hunters. If it be true, it is certainly a curious display of animal instinct. This hour is therefore always kept in view by the hunter, as he rides slowly through the forest, with his rifle on his shoulder, while his keen eye penetrates the surrounding shades. On beholding a deer, the hunter slides from his horse, and, while the deer is observing the latter, creeps upon him, keeping the largest trees between himself and the object of pursuit, until he gets near enough to fire. An expert woodsman seldom fails to hit his game. It is extremely dangerous to approach a wounded deer. Timid and harmless as this animal is, at other times, he no sooner finds himself deprived of the power of flight, than he becomes furious, and rushes upon his enemy, making desperate plunges with his sharp horns, and striking and trampling furiously with his fore-legs, which, being extremely muscular, and armed with sharp hoofs, are capable of inflicting very severe wounds. Aware of this circumstance, the hunter approaches him with caution, and either secures his prey by a second shot, where the first has been but partially successful, or, as is more frequently the case, causes his dog to seize the wounded animal, while he watches his own opportunity to stab him with his hunting-knife. Sometimes where a noble buck is the victim, and the hunter is impatient or inexperienced, terrible conflicts ensue on such occasions.—Another mode, is to watch at night, in the neighbourhood of the salt-licks. These are spots where the earth is impregnated with saline particles, or where the salt-water oozes through the soil. Deer and other grazing animals frequent such places, and remain for hours licking the earth. The hunter secrets himself here, either in the thick top of a tree, or most generally in a screen erected for the purpose, and artfully concealed, like a mask-battery, with logs or green boughs. This practice is pursued only in the summer, or early in the autumn, in cloudless nights, when the moon shines brilliantly, and objects may be readily discovered. At the rising of the moon, or shortly after, the deer having risen from their beds, approach the lick. Such places are generally denuded of timber, but surrounded by it; and as the animal is about to emerge from the shade into the clear moon-light, he stops, looks cautiously around, and snuffs the air. Then he advances a few steps, and stops again, smells the ground, or raises his expanded nostrils, as if he "snuffed the approach of danger in every tainted breeze." The hunter sits motionless, and most breathless, waiting until the animal shall get within rifle-shot, and until its position, in relation to the hunter and the light, shall be favourable, when he fires with an unerring aim. A few deer only can be thus taken in one night, and after a few nights these timorous animals are driven from the haunts which are thus disturbed.—Another practice is called *driving*, and is only practised in those parts of the country where this kind of game is scarce, and where hunting is pursued as an amusement. A large party is made up, and the hunters ride forth with their dogs. The hunting ground is selected, and, as it is pretty well known what tracts are usually taken by the deer when started, an individual is placed at each of those passages, to intercept the retreating animal. The scene of action being, in some measure, surrounded, small parties advance with the dogs from different directions, and the startled deer, in flying, most generally pass some of the persons who are concealed, and who fire at them as they pass.——The *elk* has disappeared. A few have been seen of late years, and some taken; but it is not known that any remain at this time, within the limits of the State.—The *bear* is seldom seen. This animal inhabits those parts of the country that are thickly wooded, and delights particularly in cane-brakes, where it feeds in the winter on the tender shoots of the young cane. The meat is tender and finely flavoured, and is esteemed a great delicacy. *Wolves* are very numerous in every part of the state. There are two kinds: the common or black wolf, and the prairie wolf. The former is a large fierce animal, and very destructive to sheep, pigs, calves, poultry, and even young colts. They hunt in large packs, and after using every stratagem to circumvent their prey, attack it with remarkable forocity. Like the Indian, they always endeavour to surprise their victim, and strike the mortal blow without exposing themselves to danger. They seldom attack man except when asleep or wounded. The largest animals, when wounded, entangled, or otherwise disabled, become their prey, but in general they only attack such as are incapable of resistance. They have been known to lie in wait upon the bank of a stream, which the buffaloes were in the habit of crossing, and, when one of those unwieldy animals was so unfortunate as to sink in the mire, spring suddenly upon it, and worry it to death, while thus disabled from resistance. Their most common prey is the deer, which they hunt regularly; but all defenceless animals are alike acceptable to their ravenous appetites. When tempted by hunger, they approach the farm-houses in the night, and snatch their prey from under the very eye of the farmer; and when the latter is absent with his dogs, the wolf is sometimes seen by the females lurking about in mid-day, as if aware of the unprotected state of the family. Our heroic females have sometimes shot them under such circumstances. The smell of burning assafœtida has a remarkable effect upon this animal. If a fire be made in the woods, and a portion of this drug thrown into it, so as to saturate the atmosphere with the odour, the wolves, if any are within reach of the scent, immediately assemble around, howling in the most mournful manner; and such is the remarkable fascination under which they seem to labour, that they will often suffer themselves to be shot down rather than quit the spot. Of the very few instances of their attacking human beings of which we have heard, the following may serve to give some idea of their habits: In very early times, a Negro man was passing in the night, in the lower part of Kentucky, settlement to another. The distance was several miles, country over which he travelled entirely unsettled. In ning his carcass was found entirely stripped of flesh. lay his axe, covered with blood, and all around, the were beat down, the ground trodden, and the number of ts so great, as to show that the unfortunate victim had ong and manfully. On pursuing his track, it appeared wolves had pursued him for a considerable distance, he n turned upon them and driven them back. Several hey had attacked him, and been repelled, as appeared by d and tracks. He had killed some of them before the et, and in the last conflict had destroyed several; his his only weapon. The *prairie-wolf* is a smaller species, kes its name from its habits, or residing entirely upon n plains. Even when hunted with dogs, it will make fter circuit round the prairie, carefully avoiding the fo- only dashing into it occasionally when hard pressed, and turning to the plain. In size and appearance this ani- midway between the wolf and the fox, and in colour it bles the latter, being of a very light red. It preys upon y, rabbits, young pigs, calves, &c. The most friendly re- subsist between this animal and the common wolf, and onstantly hunt in packs together. Nothing is more com- han to see a large black wolf in company with several -wolves. I am well satisfied that the latter is the jackal a. Several years ago, an agricultural society, which was lished at the seat of government, offered a large premium person who should kill the greatest number of wolves in year. The legislature at the same time offered a bounty ach wolf-scalp that should be taken. The consequence that the expenditure for wolf-scalps became so great, as to r it necessary to repeal the law. These animals, although numerous and troublesome to the farmer, are greatly de- ed in number, and are no longer dangerous to man. We w of no instances in late years of a human being having been ked by them.—*Featherstonehaugh's Journal.*

Entomology in Scotland.—The great attention which has n bestowed for many years on the Entomology of England, re there is scarcely a single city without one or more assiduous collectors, renders the fact the more remarkable, that, in the northern portion of the island, this delightful study should have made so slight a progress. This may be in some manner owing to the want of a proper elementary work, of a sufficiently compendious nature, to guide the student through the intricacies of a subject somewhat encumbered by an unsettled system of nomenclature and arrangement. We are therefore happy to have it in our power to announce that the first volume is in a forward state of preparation, of a work entitled *Entomologia Edinensis*, or a description and history of the Insects indigenous to the neighbourhood of Edinburgh, by Mr James Wilson, F. R. S. E., &c. and Mr James Duncan. This volume is intended to contain the generic characters and specific descriptions of the coleopterous insects found in the district just named, combined with a general history of their localities, economy, and metamorphoses. An introductory essay will present a general view of the Class Insecta,—pointing out its distinctive attributes and relations to the other great divisions of the animal kingdom, and including an account of the anatomical structure, physiology, geographical distribution, &c. of the extensive order, to a portion of which the descriptive part of the forthcoming volume exclusively relates.

GEOLOGY.

6. *Heights of Mountains and Lakes in North America:—*

Long's Peak Chippeweyan, or Rocky Mountains, ...	15,000 feet.
Mount Washington, N. Hampshire,[*]	6,234
Mansfield Mountain, N. Peak, Vermont, ...	4,279
Catskill Mountains, Round Top, N. York, ...	3,800
Black Hills, Lat. 40. NW. of Missouri, ...	3,500
Alleghany Mountains, in Virginia, ...	3,100
Ozarc Mountains, west of Mississippi, ...	2,250
Wisconsan Hills, S. of Lake Superior, ...	2,250
Catskill Mountainhouse, N. York, ...	2,214
Sources of streams tributary to Lakes Winnepec and Superior, ...	1,200
Head waters of the Mississippi, ...	1,200
Break Neck, near West Point Foundery, ...	1,187
Rainy Lake, SE. of the Lake of the Woods, ...	1,100
Tourn Mountain, Rammapoo, N. Jersey, ...	1,067

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[*] This is the loftiest of the White Mountains.

Lake of the Woods,	1,040
Dog Lake,	1,000
Source of the Miami,	964
Source of the Sciota,	919
Sources of the St Peter and Red Rivers,	830
Mouth of the Platte, Missouri,	680
Mouth of the St Peter, Mississippi,	630
Lake Winnepec,	595
Lake Superior,	571
Lakes Huron and Michigan,	571
Ohio, near Wheeling, Virginia,	565
Lake Erie,	565
Ohio, at Cincinnati,	414
Point Levi, opposite Quebec,	310
Mouth of the Ohio,	300
Lake Ontario,	231

Featherstonehaugh's American Journal of Geology.

MINERALOGY.

7. *On the Gold, Silver, and Platina of Russia.*—The annual produce of silver in the Russias, is estimated at about 1000 pouds, of forty pounds each; but what, after all, is this 1000 pouds, or 40,000 pounds, to the produce of the Valenciana mine in Mexico, which for many long years produced its millions of dollars annually? "Young Demidoff had not yet returned from Italy; from his relation and agent Daniloff, I met with every attention. His cabinet contained many beautiful specimens of platina, most of which were designed as presents to the crowned heads of Europe. Although some single masses of platina weighed seven or eight pounds, none could be compared to those in the cabinet of the mining corps, one of which weighed about 27 pounds. My own specimens, which were presented to me by Zobolefsky, although weighing 800 grains each, and of which I had been not a little proud, dwindled away in the view of the great varieties lying in profusion in Demidoff's cabinet. Owner of the most celebrated platina deposits, and gold-washings, he had had many opportunities, in the course of a few years, of selecting and putting aside not only large massive lumps of gold and platina, but what was yet more interesting, a great variety of most beautiful and perfect crystals of gold. The mass of platina before alluded to, as weighing 27 pounds, was found completely isolated, and at nearly 60 versts from the

usual deposits of platina, in a bed of red clay, where some slaves were employed in making bricks. Those streams in the beds and on the banks of which the gold deposits are met with, contain more of gold, and less platina, on the European than those on the Asiatic side of the Ural Mountains. The amount of gold obtained from these washings, had amounted for the year 1830, to nearly half a million sterling. It may be well imagined to what an extent their operations must be extended, when the 100 pouds, or 4000 pounds weight of soil, seldom yield above 65 grains of gold, and varies from 65 to 120 grains, —which is there considered rich,—to the 100 pouds. Nevertheless, their mining operations are conducted with such skill and success, as even to obtain, of this limited quantity, nearly the whole amount; and that, too, with such little cost, as to have been, indeed, far beneath my expectation. Of the simple and yet beautiful processes made use of in the gold-washings of the Ural Mountains, I shall speak hereafter, well convinced of the great utility and service which they would be of, if made known to the mining regions of other countries. The Demidoffs, Davidoffs, and many other Russian families, are acquiring princely revenues from the employment of their slaves in these gold-washings; but it is not alone the gold,—the platina itself is another great source of their prosperity; more especially since all the platina is now coined at the imperial mint, and established as part of the current coin of the realm. The coins made of platina are beautiful; those large pieces with the head of the Emperor are the best, and show better the effect and polish which coins of this metal can take. Though many hundred pounds weight of platina are coined monthly, into pieces of 11 and 22 rubles, they disappear rapidly from the circulation. They may be met with occasionally, and a few at a time, in the hands of the brokers. I consider their price much above the London price of malleable platina, which is at present about 25 shillings English per ounce: considering that the crude platina is the produce of the country, the Russian price for malleable platina, which is about 28s., is too extravagant; and yet this does not arise from the expense of manufacturing, but from the cost of the material itself, which is far higher than the platina of South America. The cause of this is the monopoly and easy disposal of it, at a high price, through the coinage.—*Featherstonehaugh's Journal*, September 1831.

BOTANY.

8 *Zygophyllum arboreum* of Jacquin.—This species of the Guayacan or Bean-caper tree, is a common native of the province of Carthagena in South America. It grows to the height of 40 feet, and the wood is remarkably dense and heavy, being of greater specific gravity than the most compact oak. The Spanish settlers speak with enthusiasm of its durability. It has been found by experience to be so lasting, when driven as piles into the ground, that they often give it the name of *imperishable wood.* As it does not contain any gallic acid or tannin, iron-fastenings do not act injuriously upon it. This timber, it is believed, might easily be procured; and it might be worth while to try some piles of it in our sea-piers, in the hope that it would resist the attacks of the minute but very destructive marine insect (*Lemnoria terebrans* of Leach), the ravages of which have hitherto baffled the ingenuity of our engineers.

List of Patents granted in England, from 2d August to 30th August 1831.

1831.

Aug. 2. To Sir J. C. Anderson, Bart. Bultenant Castle, county of Cork, "for certain improved machinery for propelling vessels on water, which machinery is applicable to other useful purposes."

3. To J. Hall, younger, Dartford, engineer, "for an improvement in machinery used in the manufacture of paper." Communicated by a foreigner.

10. To J. M. E: Ardit, Newman Street, Oxford Street, printer, "for a machine or apparatus for drawing, and for copying and reducing drawings and other objects or subjects, and for taking panoramas." Communicated by a foreigner.

To A. Cochrane, Esq. Norton Street, Great Portland Street, "for certain improvements in machinery for propelling or moving locomotive carriages, and giving motion to mills and other machinery."

To W. Mason, London, patent axle-tree maker, "for certain improvements in the construction of wheeled carriages."

11. To D. Selden, Liverpool, merchant, "for certain improvements

in metallic mills for grinding coffee, corn, drugs, paints, and various other materials." Communicated by a foreigner.

Aug. 13. To A. W. Gillet, Birmingham, "for a new or improved machine or instrument to measure, beat, and give the accents in all the different moods of time, with any degree of velocity required, applicable to the teaching of music." Communicated by a foreigner.

27. To J. Perkins, Fleet Street, engineer, "for his improvement on his former patent, dated July 2. 1831; making the same applicable to the evaporating and boiling of fluids for certain purposes."

30. To B. Aingworth, Birmingham, button-maker, "for an improvement in the making and constructing of buttons."

List of Patents granted in Scotland from 15*th to* 28*th March* 1832.

1832.

March 15. To Joel Benedict Nott of Liverpool, Esq. in consequence of a communication made to him by a certain foreigner residing abroad, and invention by himself, for "certain improvements in the construction of a furnace or furnaces for generating heat, and in the apparatus for the application of heat to various useful purposes," being farther improvements upon a patent obtained by him, dated the 4th day of November 1830.

To John Ericsson of Liverpool, in the county palatine of Lancaster, civil engineer, for an invention of "an improved engine for communicating power to mechanical purposes."

21. To James Thomson of Gorbals, city of Glasgow, and county of Lanark, distiller, for an invention of "an improvement on the construction of distilling apparatus, and particularly of the condenser or worm."

28. To Peter Young of Fenchurch Street, rope and sail maker, in consequence of a communication made to him by a certain foreigner residing abroad, for a new mode of "manufacturing mangel wurzel, for the purpose of producing various known articles of commerce."

To Elijah Galloway of Carter Street, Walworth, in the county of Surrey, engineer, for an invention of "certain improvements on paddle-wheels."

To Henry Warner of Loughborough, in the county of Leicester, hosier, Charles Hood of the same place, frame smith and setter up, and Benjamin Abbot, also of the same place, frame-work knitter, for an invention of "certain improvements upon the machinery now in use for making or manufacturing stockings, stocking-web, or frame-work, knitting-warp web, warp-net, and point-net."

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Memoir of WILLIAM *Roscoe*, *Esq*. By Dr THOMAS STEWART TRAILL, F.R.S.E., &c. Communicated by the Author.

> "Clarorum Virorum facta moresque posteris tradere antiquitus usitatum, ne nostris quidem temporibus, quanquam incuriosa suorum ætas omisit, quotiens magna aliqua ac nobilis virtus vicit ac supergressa est vitium, parvis magnisque Civitatibus commune, ignorantiam recti et invidiam."—TACITI *Vita Agricolæ*.

IN the sentence now quoted, Tacitus has justly indicated the true objects of biography; and, although in this humble notice of our late illustrious President[*], I do not profess the intention of handing down his character and virtues to posterity (a task fortunately confided to abler hands[†]), yet I feel satisfied, that this attempt will not be displeasing to a Society of which he was at once the ornament and the head. As our age cannot be justly accused of want of curiosity respecting our contemporaries, it does not deserve to be characterized as ignorant or envious of merit. If, in tracing the career of Mr Roscoe, we find him rising, by his own exertions, from obscurity to eminence, that age and that country have some claims to commendation in which the force of genius can overcome the obstacles of birth and fortune, and elevate its possessor to the society of the noblest and wisest of the land.

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[*] Read before the Literary and Philosophical Society of Liverpool in October 1831.

[†] The public will soon have the satisfaction of receiving from the pen of Henry Roscoe, Esq. barrister-at-law, a life of his father, illustrated by selections from an extensive and interesting correspondence with many distinguished characters of his age.

William Roscoe was born on the 8th of March 1753, in the *Old Bowling-green House*, which still exists in Mount Pleasant *, and is well known to many persons by the engraving from a drawing by Austin. His parents, in humble but comfortable circumstances, were little able to advance his education; yet anxious for his improvement, at the age of six they sent him to a school, kept by a Mr Martin, for the elementary instruction of children; whence, in about two years, he was removed to the seminary of Mr Sykes, at that time a considerable private school in Liverpool.

The instruction which young Roscoe here received was confined to English reading, writing, arithmetic, and the elements of geometry. At the age of twelve years he left school, from which period he may be said to have been, in a great measure, his own instructor, until about the age of sixteen, when he was articled as clerk to Mr John Eyes, a respectable attorney in this town. During the four years that elapsed between his leaving school and entering Mr Eyes's office, he occupied himself with desultory English reading, in cultivating some fields rented by his father, and in frequenting the painting-room of a porcelain manufactory in the neighbourhood, where he amused himself with painting on china.

At that period of his life his English reading appears to have been rather confined. His favourite authors were Shakspeare, Shenstone, the poems of Mrs Catharine Philips, and the Spectator. From the former he imbibed a decided predilection for poetry, and his taste for English composition was probably modelled on the elegant examples contained in the latter. It is curious to trace his attachment to botany and the fine arts to this early period. The phenomena of vegetation, and the cultivation of plants, appear to have made a deep impression on his youthful mind; and in the little cultivator of his father's fields, we can trace the embryo botanist, to whose ardent enthusiasm in after years, *we* owe our botanic garden, *the world* the new arrangement of Scitamineæ, and the superb botanical publication on the same beautiful order of plants. The early essays in painting china-ware seem also to have first inspired him with a love of the fine arts, and drew him on to cultivate his taste in the arts of design, in which he not only displayed the knowledge of an intelligent amateur, but such practical proficiency, as might have led to eminence, had his genius not been directed to other channels, as several slight but spirited etchings by his hand, yet in existence, amply testify.

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* A street in Liverpool.

The rudiments of Latin he acquired between the age of sixteen and twenty, by his own unassisted efforts, though at a later period he read several of the best Latin authors in company with his friends the late William Clarke and Richard Lowndes, two young men of Liverpool, equally intent with himself on mental improvement.

I may here mention, it was not until a comparatively later period of his life, and, if I mistake not, after the publication of the Life of Lorenzo had given him celebrity, that he began to study Greek. In a copy of Homer in possession of his family, we find the following note:—"Finished the Odyssey the day I came to Allerton, 18th March 1799.—W. R."

From his fifteenth to his twentieth year, he appears, from some memoranda which he has left, to have studied very assiduously during his leisure hours; and he luckily found some associates, with congenial tastes and habits, of whose friendship he always spoke, to his latest hour, with affectionate regard. Among those the most conspicuous were Mr Edward Rogers, Mr William Clarke, Mr Richard Lowndes, Mr William Neilson, and Mr Francis Holden. To the latter, whose various acquirements and extraordinary talents were in after life the frequent theme of Roscoe's enthusiastic encomiums, he was disposed to attribute his first inclination to the study of modern languages; and he had pleasure in acknowledging, that it was by the advice and encouragement of this young friend, that he devoted himself assiduously to the study of Italian. In his acquisition of the elements of French and Italian, he does not seem to have had any other assistance than the advice and encouragement of young Holden, who, seeing the aptitude and industry of his friend, strenuously urged him to pursue the path which his own genius had opened to his aspirations after literary distinction.

In fact, Roscoe owed very little of his acquirements to any instructor. What he drew from the conversation of his early associates, there is every reason to believe he amply repaid in kind; and, with the single exception of Burns, I do not know any of our distinguished writers who is less indebted to others for assistance in the road to literary eminence than WILLIAM ROSCOE—certainly few of them could with more truth exclaim, in the language of *Phemius*, when a suppliant to Ulysses,—

῎Αυτοδιδακτος δ'ειμι.

During the time of his apprenticeship, Mr Roscoe formed an agreement with his friends Clarke, Lowndes, and Holden, to meet early in the morning, before the hours of business, for the purpose of reading together some Latin author, and discoursing on what they read. The example of these youthful students cannot be too earnestly inculcated on the rising generation of this place, while the success of one at least of them in the fields of literature is a striking proof of what may be obtained by such appropriation of hours too often lost to mental improvement. While classic authors thus engaged his morning leisure, Roscoe continued earnestly to cultivate Italian literature. It would seem that, before his twentieth year, he had read, in the original, several of the Italian historians, and, at that time even, had set his mind on becoming the biographer of Lorenzo de' Medici, the great patron of the early restorers of ancient learning.

He had, from an early period of his life, felt the force of poetic inspiration, and had undoubtedly cultivated the Muses with high promise of brilliant success, ere he had attained his twentieth summer. A considerable number of his early verses remain, which breathe an ardent spirit of poetry. Some of these are addressed to a young lady of the same age, whose poetical genius had excited his warmest admiration, and who appears to have no less admired the talents of Roscoe. One of her MS. poems, written about 1772, contains the following lines, which at once prove the poetic powers of the author (destined afterwards to become the mother of an eminent poet), and shew her discernment in detecting, in the unknown attorney's clerk, the germ of future eminence:—

> "But cease, my Muse, unequal to the task,
> Forbear the effort! and to nobler hands
> Resign the lyre! Thee Roscoe! every Muse
> Uncalled attends, and uninvoked inspires:
> In blooming shades and amaranthine bowers
> They weave the future garland for thy brow,
> And wait to crown thee with immortal fame;—
> Thee Wisdom leads in all her lovely walks,
> Thee Genius fires, and moral beauty charms;
> Be it thy task to touch the feeling heart,
> Correct its passions, and exalt its aims;
> Teach pride to own, and owning to obey
> Fair Virtue's dictates, and her sacred laws;
> To brighter worlds shew thou the glorious road,
> And be thy life as moral as thy song."

Congeniality of disposition and genius drew close the bands of friendship between this lady and Mr Roscoe; and it is worthy of notice, that his first published production, the poem entitled *Mount Pleasant*, written at the age of nineteen, was originally inscribed to her, although, when printed, the address was omitted.

This poem, which exhibits, with considerable power of versification, a warm poetical feeling of the beauties of nature, is still more remarkable for the indignant apostrophe to Britons on the slave-trade; an expression of generous sympathy with the suffering sons of Africa, which it required no inconsiderable share of moral courage to promulgate at that period, and in the chief seat of the odious traffic.

> "Shame to mankind! But shame to Britons most,
> Who all the sweets of Liberty can boast;
> Yet, deaf to ev'ry human claim, deny
> That bliss to others which themselves enjoy:
> Life's bitter draught with harsher bitter fill,
> Blast ev'ry joy, and add to ev'ry ill;
> The trembling limbs with galling iron bind,
> Nor loose the heavier bondage of the mind."

These lines are here chiefly quoted to shew how early Roscoe denounced the traffic in human flesh; and that the love of liberty which marked his whole life, was in him, not the creation of circumstances that brought him into public notice, but had grown up with his expanding faculties, and became confirmed by the reflections of his maturer years.

While Roscoe was thus improving his literary taste in the moments snatched from the fatigues of his profession, he devoted no inconsiderable portion of his attention to the study and promotion of the fine arts in his native town. He was the chief instigator, and most active member, of a small society formed here in 1772, "*for the encouragement of designing, drawing, and painting*;" and he read before that association, at one of its first meetings, an ode, addressed to the institution, which was afterwards published with the poem of *Mount Pleasant*, in 1777.

Soon after the termination of his articles of clerkship, Mr Roscoe entered into partnership as an attorney with Mr Aspinall; and in this profession he continued, first with that gentleman, and afterwards with Mr Joshua Lace, until the year 1796. It is proper to remark, that Mr Roscoe, though eminent as a practitioner, never relished his profession, and had always expressed his determination to retire from practice as an attorney, whenever the possession of a moderate competence should enable him to devote his attention to literary pursuits. He continued, however, for several years to attend sedulously to his business; but it was not until the year 1781 that the profits of his exertions enabled him to marry; when he was united to Jane, the second daughter of Mr William Griffies, a respectable tradesman of Liverpool.

This union was productive of the utmost domestic happiness to the subject of this memoir, and made him the happy father of seven sons and three daughters; all of whom, except a son and a daughter, survive him.

For several years after his marriage, at intervals of leisure, he contrived to increase those rich treasures of literary information, and to cultivate that taste for the fine arts, which gave a peculiar charm to his conversation, and paved the way to his future celebrity.

In the year 1787, the agitation of the abolition of the slave-trade drew Mr Roscoe into the field of political controversy; and he became the author of two anonymous pamphlets on that great question. The first was entitled "*Original View of the African Slave Trade, demonstrating its injustice and impolicy; with hints toward a Bill for its abolition.*" The second was called forth by the publication of the Rev. Raymond Harris, entitled "*Scriptural Researches on the Licetness of the Slave-Trade.*" That author had shewn much dexterity as a controversialist. The general scope of his argument is based on the practice of possessing *bond-servants* being mentioned in the Old Testament, without any condemnatory comment; and from the noted injunction of St Paul to the Δουλοι of the New Testament, "*Be obedient to them that are your masters, according to the flesh, with fear and trembling,*" it is inferred, that Christianity gives a warrant for holding our fellow men in slavery. The first proposition was supported by the fact, that Abraham and other very exemplary patriarchs purchased slaves or bond-servants without any stigma being cast on their humanity or rectitude; and the second was defended by the repeated injunctions of the apostles to their converts to fear those in authority; a mode of reasoning which has been lately borrowed by two Presbyterian divines from this reverend Jesuit; whose work, it is said, was considered, by the Common Council of Liverpool of that day, as worthy of a donation of L. 200 to the author.

Harris's pamphlet caused a considerable sensation; but was soon attacked by the Rev. Mr Dannet, minister of St John's, in Liverpool, who was evidently inferior as a controversialist to his Catholic opponent; but the doctrines of the latter were ably and warmly attacked in Mr Roscoe's second essay, which bore the title of "*Scriptural Refutation of a Pamphlet lately published by the Rev. Raymond Harris, &c.*" on the Christian principles that "all men are equal in the sight of God," and the benevolent injunction of the great Founder of our religion, "Therefore, all things whatsoever ye would that men should do to you, do ye even so to them."

The question of the slave-trade, at that period, so engrossed the mind of Roscoe, that, in the same year, he published his well known poetic effusion, "The Wrongs of Africa," in two parts; the profits from the sale of which he placed at the disposal of the committee then formed for promoting the abolition of the slave-trade

The Society for the encouragement of the Arts of Design had soon melted away; but Mr Roscoe, about the time now alluded to, succeeded in forming a new association for a similar purpose; and to their exertions Liverpool is indebted for its first exhibitions of works of art, which were continued for several years with great success. To this Society, which bore the name of "Liverpool Academy for the Encouragement of the Fine Arts," Mr Roscoe delivered a series of lectures on the Progress and Vicissitudes of Taste, which remain in manuscript, and which he appears to have, at one time, contemplated to publish, as I find among them a title-page, thus—"*An Historical Inquiry into the Rise, Progress, and Vicissitudes of Taste, as exemplified in Works of Literature and of Art. In two volumes. Vol. I.*" The manuscript, however, does not seem to have received his last corrections; though many of the observations are original and interesting.

He had also, for some years, a correspondence with Mr Strutt, the engraver and antiquary, and author of the valuable *Dictionary of Engravers*. The letters of Strutt acknowledge the receipt of various important disquisitions on the history of engraving from Mr Roscoe, of which, if I mistake not, Mr Strutt availed himself, and incorporated them in the preliminary essays to his Dictionary.

About the same period, Mr Roscoe commenced his fine collection of prints, which was particularly rich in painters' etchings and engravings of the old masters. This collection was chiefly formed between the years 1780 and 1790; but continued for many years to receive valuable additions, by every journey to London, which his professional business often rendered necessary.

At a later period he began to collect drawings by great masters, and of these his collection was remarkably choice; his taste and judgment in that department being excellent.

From the time when his professional exertions put it in his power to indulge his elegant propensities, the formation of a library became a prominent article in his expenditure. Simple and refined in his habits, these were his chief expenses. He was not, however, a mere collector. His books, his prints, and other works of art, were diligently employed to store his mind, or to improve his taste, with the wisdom and art of former ages.

In the year 1789, after much previous study of Italian literature, he began to devote himself to the object of his early ambition, the *Life of Lorenzo the Magnificent*. In the course of that year he communicated his intention to his valued friend William Clarke (who, on account of his health, in the autumn of 1789, had fixed his residence at Fiesole, near Florence), and requested his assistance in collecting manuscript documents relating to the subject. The fruits of the friendly exertions of that amiable man are best given in Roscoe's own words.

—" An intimate friend, with whom I had been many years united in studies and affection, had paid a visit to Italy, and had fixed his winter residence at Florence. I well knew that I had only to request his assistance, in order to obtain whatever information he had an opportunity of procuring, from the very spot which was to be the scene of my intended history. My inquiries were particularly directed to the Laurentian and Ricardi Libraries, which I was convinced would afford much original and interesting information. It would be unjust merely to say that my friend afforded me the assistance I required; he went far beyond even the hopes I had formed,—and his return to his native country was, if possible, rendered still more grateful to me, by the materials which he had collected for my use *."

Of these documents several are published entire in the appendix to Roscoe's work, especially the poems of Lorenzo; the existence of which had escaped the knowledge of the former biographers of the *Princely Merchant.*

From a very early period, Mr Roscoe had taken a deep interest in political matters. In the year 1788, he took an active part at the meeting which, in Liverpool, as well as in other parts of England, assembled to commemorate the centenary of the Revolution that expelled the family of Stuart from the throne of these kingdoms; and he composed an ode, which was recited on that occasion.

In the following year the French Revolution broke out; and, in common with many warm and generous spirits, he hailed its fair and auspicious dawn with all the devotion of a friend to the human race, the ardour of a patriot, and the enthusiasm of a poet. To those who recollect the flattering commencement of that extraordinary movement, it is unnecessary to observe, that it was viewed with unmixed satisfaction by a great majority of the people of this country, as affording the prospect of vast improvements in the social institutions of the European commonwealth. In various places meetings of the friends of liberty were held, similar to those of the preceding year, to celebrate another triumph of a great people over an unjust and tyrannical government; and at one of those assemblies Roscoe produced his two admirable lyrics—

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* Preface to Lorenzo.

> "O'er the vine-cover'd hills and gay valleys of France,"

and,

> "Unfold, Father Time, thy long records unfold."

These brilliant and exulting strains were poured forth in the year 1789, while "the Genius of French freedom," in the nervous language of Currie, "appeared on our southern horizon with the countenance of an angel,"—and ere she had yet "assumed the features of a demon, and vanished in a shower of blood."

Mr Roscoe had, on several occasions, made himself conspicuous by his attachment to the cause of civil and religious liberty, in such a degree as to attract the notice of several eminent statesmen, and particularly of the late Marquis of Lansdowne, with whom he maintained a close correspondence until the death of that nobleman. Literature, and especially politics, were the subjects of their correspondence; and the letters show how considerable were the parliamentary reforms advocated by the Whig statesmen of that period.

The violence of the second French National Assembly, in the two succeeding years, alienated a great many of their admirers in this country; but many good men still hoped that the ferment would subside into rational liberty, and deprecated the evident hostility which our government began in 1792 to exhibit. Among the latter was Mr Roscoe. On the appearance of Mr Pitt's famous proclamation against sedition, the minds of men were much agitated, and greatly divided. The friends of the minister in Liverpool convened a meeting to thank his Majesty for the proclamation. Mr Roscoe, seconded by the late William Rathbone, succeeded in carrying a counter-address;

but, on the following day, a mob rose and destroyed the counter-address where it lay for signature. Party spirit, on that occasion, rose so high in Liverpool, that a small private literary society, of which Mr Roscoe, Mr Rathbone, and Dr Currie were members*, thought it expedient to discontinue their meetings, lest their objects should, by party malice, be represented as seditious or revolutionary.

On the breaking out of the war with France, Roscoe again appears as a political writer. He inveighed against the unjust and impolitic interference of this government with France; and, in a pamphlet entitled "*Thoughts on the late Failures*," published in 1793, he attributes the mercantile distress of that period to the consequences of our meddling policy, a subject which he resumed in 1796, in "*An Exposure of the Fallacies of Mr Burke's*" celebrated invectives against the French Revolution.

We come now to the principal event in the history of our author, the publication of "*the Life of Lorenzo de' Medici*," which appeared in the winter of 1795, in two volumes quarto. The work was printed by John M'Creery in Liverpool, and is a fine specimen of provincial typography, both for accuracy and elegance of execution.

The sensation produced by this work was immense; the first edition was rapidly exhausted, and a second was demanded by the public within a few months. Letters of the most gratifying kind were showered on the author from high literary authorities in all quarters. Among others, the late Earl of Bristol, Bishop of Derry, then resident at Rome, hailed with the highest encomiums the appearance of an English work, which was the surprise and envy of the Italians themselves; and he immediately wrote to the publisher to know "what present of Italian books would be most acceptable to the accomplished author." Its success on the continent was no less gratifying. Besides a reprint of the original, the work was speedily translated into the Italian, French, and German languages; and it procured for Roscoe the esteem and correspondence of some of the most eminent literary men of Europe.

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* Besides these gentlemen, it consisted of the Rev. Joseph Smith, the Rev. John Yates, Mr Ralph Eddows, Mr Tattersall, the Rev. William Shepherd, and Dr Rutter.

A work which has received such marked public approbation, which has gone through so many large editions in Britain, which has been translated into the most polished tongues of the continent, it is not my intention now to criticise. I shall content myself with remarking, that the author's intimate acquaintance with the literature of Italy has excited the surprise even of Italians; and I well recollect the incredulous stare with which an Italian nobleman, of great literary taste and information, received my assertion that Mr Roscoe had never been out of England.

The success of the Life of Lorenzo appears to have confirmed the author's intention of relinquishing his practice as an attorney; and, some time after that event, he entered his name as a member of Grey's Inn, with a view of being called to the bar. This determination, however, he also relinquished, upon keeping a few terms.

In the year 1797, while his name remained on the books of Grey's Inn, he paid a visit of some length to London. In consequence of his literary reputation, and his intimacy with Lord Lansdowne, he was introduced into the first literary and various political circles: in particular, he used to state, that he then had the pleasure of becoming personally acquainted with Mr Fox and Mr, now Lord, Grey; and he formed besides many valued private friendships, which were dissolved only by death. Among those who then more particularly gained his esteem he often mentioned Dr Moore, author of *Zeluco*, of the spirited "*View of Manners in Italy*," and of a "*Journal of a Residence in France*,"—the father of the gallant and unfortunate Sir John Moore.

Mr Roscoe's retirement from professional labours enabled him to devote himself with increased assiduity to Italian literature. To relieve his mind from the fatigue of more intense researches, he this year translated into English verse the *Balia* of Tansillo, in which the long neglected beauties of the Italian poet are brought home to British ears and British feelings with admirable tact and spirit. His more arduous occupations were the vast stores of Italian history about the period of the restoration of Letters, with a view to the Life of Leo X.; a subject which had been recommended to him by Horace Walpole (Lord Orford), and some other literary friends, after his successful publication of the Life of *Lorenzo the Magnificent.*

In the year 1798, the want of a public reading-room on a better footing than Liverpool then could boast, caused the foundation of our Athenæum. The plan suggested by Dr Rutter was warmly supported by Mr Roscoe, Dr Currie, Mr George Case, and some other gentlemen, who, exerting their influence among their friends, obtained so general a subscription in Liverpool, that the foundations of our magnificent Consultation Library and News-Room were speedily laid, and a considerable collection of books soon formed. In this institution, as creditable to his native town, Mr Roscoe always took much delight.

The numerous strangers who were now attracted to Liverpool, chiefly by the reputation of our distinguished fellow-citizen, caused such encroachments on his time, notwithstanding his retirement from business, that he resolved to retreat into the country; and with this view he purchased half of the estate of Allerton from the representatives of Mrs Hardman, and removed to that beautiful spot in 1799. In a playful letter to Fuseli the painter, Mr Roscoe mentions his removal, and his intention of not again embarking in any kind of business, but of dedicating himself wholly to agricultural and literary pursuits. His tastes were simple, his views moderate, and his means fully competent to realize his plans; from which it is greatly to be lamented that any circumstances should ever have induced him to deviate. While employed as a professional man to arrange the involved concerns of the bank of Messrs J. & W. Clarke, he was thus brought into contact with Sir Benjamin Hammet, a London Banker, who held acceptances of the Liverpool Bank to an immense amount (I am informed for L. 200,000). Hammet was so struck with Roscoe's ability in arranging the affairs of his friends, that he wished him to become a partner in the concern. This he repeatedly refused; but Hammet threatening, in case of his refusal to join the concern, to make it bankrupt, Roscoe, satisfied that the assets were, in ordinary times, more than sufficient to cover the demands against the bank, finally consented, and for twenty years the principal part of his time was occupied in the management of that important establishment.

While thus employed, the hours which he was now enabled to devote to the history of Leo were abstracted from the period usually dedicated to repose or recreation. Yet, with all these demands on his application, the interest he always felt on great political questions did not suffer him to view in silence the crisis of 1802; but called forth his pamphlet entitled "*Observations on the relative situation of Great Britain and France*;" a tract in which he has recorded his detestation of war, and his anxiety to see the two foremost nations of Europe engaged in the less guilty rivalry for pre-eminence in the arts of peace.

In 1802 the Botanic Garden of Liverpool was established, chiefly through the influence of Mr Roscoe, and, at its opening, he delivered an address to the proprietors, which was printed. In this establishment he always took the interest of a parent in a favourite child. Under his auspices, and the consummate skill of Mr John Shepherd, the Curator of the garden, it speedily became conspicuous among botanical establishments, and it still ranks among the first in Europe.

In 1805 he completed his history of "*the Life and Pontificate of Leo. X.*" which appeared that year in four volumes 4to.

This elaborate work had been the fruit of much research, of intense previous study, and was always regarded by its author as superior to his Life of Lorenzo: yet it was not so favourably received by the British public; a circumstance with some truth attributed to the violent attacks on it in several of our periodical works.

The hostility of some of the reviews was evidently produced by political rancour toward Roscoe as an opponent of the ministerial measures of that eventful period; in others it sprung from anger at the manner in which he had treated the character of Luther as the founder of a new church.

It appears to me that Roscoe had sufficiently lauded the boldness and constancy of the man to whom we owe the assertion of private judgment in religious and civil matters; that he had given the history of Luther with the impartiality of a searcher after truth, admitting his failings and his errors, while he applauded his courage and undoubted talents; that the early patrons of literature and the fine arts demanded our warm approbation and our gratitude, and that the historian of the Revival of Letters had wisely dwelt more on the unquestionable merits of Leo and his family, in promoting that great object, than in raking up, from the gross scurrility of a profligate age, imputed crimes or vices, which, after all, rest only on the doubtful authority of acrimonious and vindictive *controversialists*.

The merits of the Life of Leo were, however, differently estimated on the Continent. It was speedily translated into French, German, and Italian; and the extensive sale of several editions of it in Italy, where, it may be fairly presumed, the best judgment of its wants or its defects could be formed, sufficiently attests the character which foreign critics entertain of this great work of our author.

On the appearance of the British criticisms, Mr Roscoe prepared a full answer to the objections of the reviewers, but this reply he never published; contenting himself with inserting a short abstract of it in the preface to the second edition, which appeared in 1806, within a year from the publication of the first. The chief objections, in addition to the criticisms above stated, were, that he was fond of paradoxical opinions,—as instanced in the chivalrous defence of the character of Lucretia Borgia,—and in his doubting whether Pope Alexander VI. was stained with every crime laid to his charge by his numerous enemies,—that he was too fond of quotations from the poets,—and that by retaining Italian orthography of proper names, he had made needless innovations on our language. These charges are not very important defects in such an elaborate work, even if we admit their justice. In my opinion, they are sufficiently answered by his few remarks; and the Life and Pontificate of Leo X. already ranks, by the beauty of its style, and the value of research, among our standard historical works.

While engaged in these occupations, on the dissolution of Parliament in October 1806, Mr Roscoe was to himself most unexpectedly called on to become a candidate for the representation of his native town. The requisition was signed only a few days previous to the election:—and, after a keen contest for eight days, during which, there was a coalition against him, between the friends of the rival candidates Generals Tarleton and Gascoyne, he was placed at the head of the poll by a large majority.

His parliamentary career was of short duration, but he had the satisfaction, in that short period, of declaring his sentiments on several subjects in which he felt a deep interest. He spoke and voted for Sir Samuel Romilly's bill *for rendering real estates subject to simple contract debts;* he had the happiness to lift his voice in Parliament for the abolition of the slave-trade, and to see that great act of national justice triumphantly carried; he had an opportunity of advocating the claims of our Roman Catholic brethren to an equality of political rights; and he delivered his sentiments with indignant energy on the dismissal of the Whig administration in 1807, on their attempt to redeem the pledge given by Pitt at the period of the Irish Union; a pledge by themselves always considered as just and expedient.

Mr Roscoe's chief parliamentary friends at that time, were, Mr Whitbread, Sir Samuel Romilly, and Mr William Smith; but he never permanently attached himself to the ministry; and was by them regarded as a person who would rather act on his own views of what was right, than enter into the trammels of party.

On the dissolution of Parliament, he received another requisition to offer himself for Liverpool, and was escorted into town by a very numerous and respectable *cortege.* His opponents, however, had succeeded in prejudicing many of the populace against him, especially on the ground of his vote for the abolition of the Slave-Trade—a traffic which they had been taught to consider as essential to the commercial greatness of the port; and the rage of zealots was kindled against him for his speech on the Catholic Question. The consequence was, that, on the arrival of the *cortege* in Castle Street, a serious riot took place, and Mr Roscoe was induced, from the fear of hazarding the peace of the town, to decline allowing himself to be again put in nomination. I have reason to believe that this determination did not cause him much regret; a distaste for parliamentary duties was not unnatural to a man of his previous habits, entering on a new career at a rather advanced period of life. These considerations determined him to withdraw from the contest; but he was, without *his* concurrence, or that of his most intimate friends, proposed as a candidate on that occasion, as well as in 1812, when he was put in nomination in a similar manner at Leicester without his consent, and polled a very considerable number of votes.

We have already noticed Mr Roscoe as the earnest advocate of peace. In 1808, he published "*Considerations on the Causes and Consequences of the War with France*,"—a pamphlet which excited much attention, and speedily went through eight editions. It was followed in the same year by another pamphlet, entitled "*Remarks on the Proposals made to Great Britain for opening Negociations for Peace with France*," in which he endeavoured to shew that the advances of France had not been met with a sincere desire on the part of our Government to put an end to the miseries of a ruinous and bloody contest.

In 1810, Mr Roscoe published a Letter to the present Lord Chancellor, then Mr Brougham, on the question of *Parliamentary Reform*. In that tract he advocates a more extensive Reform than the partial measures then in contemplation by the Opposition; and the coincidence, in many respects, between his suggestions and a late measure, has, within the last few months, caused a republication of Mr Roscoe's pamphlet.

This letter brought him into more immediate correspondence with Mr Brougham; and when that gentleman was invited to become a candidate for the representation of Liverpool in 1812, Mr Roscoe not only entered warmly into the contest to support Mr Brougham, but, on the return of Mr Canning, the rival candidate, wrote a caustic review of the electioneering speeches of that statesman, which the enthusiasm of his admirers had collected into a bulky pamphlet. Such ephemeral specimens of oratory should, on all sides, be permitted to remain in the less ambitious pages of provincial newspapers.

At every period of his life Mr Roscoe was much attached to the study of Botany. As we have stated, the establishment of our Botanic Garden was principally due to his suggestions. In

1809, he presented to the Linnean Society his valuable paper "*On a new arrangement of the Scitaminean order of plants*," which appeared in their Transactions, and established his claim to the character of an original thinker in this elegant department of natural history. His reputation, still more than the claims of private friendship, led Sir James Edward Smith to institute the genus *Roscoea*, which now contains many species of that beautiful order.

A similarity of political principles, and congeniality of taste for agricultural improvements, had for some time made Roscoe acquainted with Mr Coke of Norfolk. In 1814, he was invited to visit Holkham, the splendid seat of that eminent agriculturist. There he found ample employment in the magnificent library, collected by the late Lord Leicester, uncle to the present possessor, a nobleman who, with vast wealth, possessed a highly cultivated mind, and a passion for collecting books and manuscripts. It was well known that the collection was immensely rich in classical manuscripts and unpublished works on Italian history. Mr Roscoe readily undertook the examination of this superb collection, which had afforded to Drakenborck the manuscript copies of Livy employed in his valuable edition of the Roman Historian, and which, among 600 manuscript volumes of ecclesiastical annals and Italian civil history, was discovered by Mr Roscoe to contain one of the lost volumes of Leonardo da Vinci's *Treatises on Mechanics*, and the long deplored and precious volume in which Raffaello, at the desire of the Pontiff, had made *pen drawings* of the remains of ancient Roman magnificence, illustrated by short descriptions in his own handwriting. Mr Roscoe undertook to make a *catalogue raisonnée* of the manuscripts of the collection —a task which he some years afterwards, with the assistance of Mr Madden, now one of the librarians of the British Museum, fully accomplished. This catalogue (of which a short account was given to this Society a few years ago) extends to four or five thick folio volumes, and is enriched with engraved *fac similes* and illuminated ornaments.

The manuscripts had been little attended to for many years. Most of them were in the original coarse paper covers, and some were injured by damp and time. The whole were some time afterwards consigned to Mr Roscoe's care, who put them into the hands of our eminent binder the late Mr John Jones, who, by great industry and skill, succeeded in restoring crumpled vellum to its original smoothness, in pasting torn leaves with wonderful neatness, and who bound the whole collection in a durable and elegant manner. An ancient and admirable Hebrew manuscript of the Pentateuch *, written in a beautiful hand, on deer-skins, forming a roll thirty-eight feet in length, was mounted, by the same ingenious artist, on rollers ornamented with silver bells, under the direction of a learned Rabbi, who believed the manuscript to be an eastern transcript of great antiquity.

Toward the close of 1815, by one of our too frequent commercial convulsions, and by the extent of their accommodations to persons engaged in business, the affairs of the bank in which Mr Roscoe was a partner became involved, and the house found it necessary to suspend payments. For four years Mr Roscoe devoted himself to the arrangement of their affairs; entertaining throughout the most sanguine hopes of being able finally to discharge all their engagements, as the joint property of the partners was valued, at the time of the suspension of payments, at considerably more than the amount of their debts. The depreciation, however, of that property, combined with other circumstances over which Mr Roscoe had no control, prevented the accomplishment of his most earnest wishes, and in 1820 he became a bankrupt. Previous to this (in the year 1816), his noble library, his fine collection of prints and drawings, and his curious collection of paintings, were dispersed, and the proceeds of the sale were applied to the payment of the debts of the house. It will convey some idea of the collection to state, that the books, consisting of about 2000 works, sold for no less a sum than L. 5150; the prints for L. 1886; the drawings for L. 750; and the pictures for L. 3239; making a total of L. 11,025.

The beautiful sonnet written by Mr Roscoe on parting with his library, was given to a friend, and handed about in manuscript; but the Reverend William Beloe has since inserted it,

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* Believed to be more than 1000 years old.

without any acknowledgment, in his autobiograph, as the motto to one of the chapters of that conceited work*.

Several of Mr Roscoe's friends, anxious to preserve to him various works, which they knew he highly prized, either for their intrinsic worth, or as the gift of esteemed friends, bought them up at the sale of his library, to the amount of L. 600, and presented them to Mr Roscoe. The gift, however, was firmly but gratefully declined; and the subscribers resolved to present the collection to the Athenæum Library, to be kept together as a testimony of their esteem for their respected friend; and of that library the collection now forms a distinct part.

A selection from his pictures, comprizing specimens of art highly illustrative of the progress of painting, was purchased by several of the same gentlemen, at a liberal price fixed by Mr Winstanley and myself, and presented to the Royal Institution by those admirers of Roscoe. This collection cost L. 50, and forms an interesting part of the objects which attract strangers to our Institution.

I may here remark, that the original plan of the Liverpool Royal Institution originated, and was drawn up by me in 1813; although it was carried into effect during my absence on the Continent in 1814. Mr Roscoe took an active part in this measure, was long the Chairman of the Committees while it was struggling into existence; and, as its first President, read an eloquent address on the opening of the Institution in the year 1817.

From an early period, Mr Roscoe had been a warm advocate for a reformation of the sanguinary penal code of this country.

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* SONNET.—

"As one who destined from his friends to part,
Regrets their loss, yet hopes again erewhile
To share their converse, and enjoy their smile,
And tempers as he may Affliction's dart,—
Thus, lov'd associates! chiefs of elder art!
Teachers of wisdom, who could once beguile
My tedious hours, and brighten ev'ry toil,
I now resign you—nor with fainting heart;
For pass a few short years, or days, or hours,
And happier seasons may their dawn unfold,
And all your sacred fellowships restore;
When, freed from earth, unlimited its powers,
Mind shall with mind direct communion hold,
And kindred spirits meet to part no more."

His humanity and amiable mind revolted from the frequency of executions; and he eagerly desired to see those statutes which awarded death for trifling offences, and are too barbarous to be enforced in the present day, expunged from the code of British jurisprudence. Shortly before the period of his misfortunes, his attention had been turned to the subject of penal law and prison discipline. In 1819 he published his tract, entitled "*Observations on Penal Jurisprudence, and the Reformation of Criminals*;" which was followed, between that period and 1825, by two other dissertations on the same subject.

The principle of the system which he advocates is, that the only legitimate object of punishment is the prevention of a repetition of the crime, by a reformation of the offenders; which effect he proposed to accomplish by hard labour in penitentiaries, and by moral instruction. He denies that we have any right to punish for the mere benefit to society of the example. He inveighs against the barbarous maxim that revenge or expiation for the injury committed ought ever to be the principle of penal legislation; and cannot admit that retribution to the injured party can be the proposed end of punishment. In the third part of his essay, he seems to doubt the propriety of the punishment of death in any case,—Part iii. p. 106; but, at all events, he considers that it should be reserved for four or five crimes of the blackest dye. These essays contain the outline of some principles which are now generally acknowledged; and if the humanity and generous spirit of the author have led him to form a too favourable estimate of human nature, and to overlook some difficulties in the practical application of his principles of legislation, we cannot but admire the benevolent enthusiasm and earnest appeal to the best feelings of our nature which are stampt on every page of his treatise.

In his second tract he had pointed out the evil consequences likely to result from too great severity in prison discipline, and had entered a warm protest against the horrid punishment of long-continued solitary confinement, as a general measure for effecting this reformation of offenders. He severely commented in the third part on the atrocities which appeared to have been perpetrated in the Auburn Penitentiary in the State of New York. This drew him into a long controversy with several American writers in the latter years of his life; and to the zeal with which he devoted himself to plead the cause of the outcasts of society, we have to ascribe the first serious shock to his general health, as I shall by and by have occasion to notice; but he had the happiness to find, after much angry discussion in the newspapers of the western world, that his arguments against solitary confinement, which he stigmatized as the utmost refinement of cruelty, and utterly ineffectual as a punishment, were not lost on the Americans. The infirm state of Mr Roscoe's health at that time brought me much into contact with my venerable friend; and when he learnt from various quarters that the change which was taking place in the prison discipline of America was in no small degree attributed to his expostulations, I heard him repeatedly declare, "that no literary distinction had ever afforded him half the gratification he received from the reflection on the part he had taken on this great question; and he expressed his satisfaction that he now might be permitted to think that he had not lived altogether in vain."

I have, in noticing the conclusion of this controversy, anticipated some events of his life to which we must now return.

When released from the harassing cares of business, the mind of Mr Roscoe, with the elasticity and application of youth, diligently entered on various literary projects. Since the first appearance of the Life of Lorenzo, he had obtained from Italy, and elsewhere, various documents illustrative of that work. These he prepared for publication, together with some strictures on the manner in which the character and biography of Lorenzo had been treated by Sismondi and some other writers. This work appeared in an 8vo. volume in 1822, under the title of "*Illustrations of the Life of Lorenzo de' Medici.*" The strong terms in which Sismondi accused Mr Roscoe of partiality to the Medici Family, and of palliating their crimes, drew forth an able and indignant answer; yet, it is pleasing to reflect, that when Sismondi, a few years afterwards, visited England, Mr Roscoe formed with him a personal acquaintance in no way affected by their literary controversy.

About the same period, Mr Roscoe published an amusing "*Memoir of Richard Roberts,*" a self-taught linguist, well known in Liverpool by the extraordinary number of languages which he can read, no less than by the filth of his person. The profits of this publication Mr Roscoe humanely dedicated to the use of this singular person; whose intellect, defective in every thing but language, renders him as helpless as a child; and Roberts may now be seen in whole clothes, with his portable library stuffed, as in former times, between his shirt and his skin; for he disdains a fixed abode.

An application having been made to Mr Roscoe to become the editor of a new edition of Pope's works, and to furnish a fresh life of the author, Mr Roscoe engaged in it with all the ardour of a poet, having ever been a warm admirer of Pope's genius. This was no trifling task—for he added notes on the poems with much care; and in the life, which forms the first volume of that edition, defended the talents and character of Pope from sundry imputations cast on him by Mr Bowles and others. The date of publication was 1824.

About the same time, he superintended a new edition of the *Lives of Lorenzo and Leo*, to the latter of which he added many new notes.

In 1824, Mr Roscoe was elected a "Royal Associate" of the Royal Society of Literature, founded by his late Majesty George IV. A pension of L. 100 a-year was awarded to each of ten associates, which Mr Roscoe enjoyed for three or four years; but which, from the neglect of providing a permanent fund for the purpose, would have ceased about the time of his decease. The great gold medal of the Society, value 50 guineas, was also awarded to him as an historian, two years before he died; and it remains with his family.

It would be unjust to omit, that the misfortunes of our distinguished fellow-citizen, called forth the warm sympathy of his numerous friends, and prompted them to take steps for securing him against their immediate consequences. It is more necessary to state this, because many unjust imputations have been vented against the inhabitants of Liverpool, on account of their supposed neglect of Mr Roscoe in his adversity. There was considerable delicacy necessary in the steps which were taken to testify their esteem and attachment. Mr Roscoe had a noble and independent mind. He had steadily refused the proffered gift of a valuable selection from his library, even after it had been for that purpose bought by his friends at the sale; and those who had the pleasure of being intimate with him, well knew how necessary it would be to keep him in ignorance of what was intended, until it was accomplished. During a second visit which he made to Holkham, a private fund was quickly subscribed among his friends, for the purchase of an annuity on the lives of Mr and Mrs Roscoe. The delicate task of communicating what was done devolved on me; and in the correspondence which ensued between us, though his pride of independence was at first alarmed, the example of his friend Charles James Fox, under similar circumstances, was successfully urged, to reconcile his mind to receive this spontaneous homage to his talents and his worth, from sincerely attached friends.

We have already noticed Mr Roscoe's early attachment to botany, and his critical labours on the order *Scitamineæ*, to which he had long paid much attention, stimulated by the continual additions this order was receiving from the East and West Indies. The number of new species which the judicious care of Mr Shepherd, the skilful curator of our Botanic Garden, had successfully cultivated, together with the dried specimens which Mr Roscoe received from various quarters, determined him to publish a work containing coloured figures of new or interesting species, with botanical descriptions. This gave rise to the most splendid botanical work that ever issued from the provincial press of any country; which occupied much of his time during the latter years of his life, and was only completed shortly before his death. Of this superb work, he printed too few copies; and before the second number came out, there was a necessity of reprinting additional copies of the first. The work is highly prized by botanists; and is particularly valued on the Continent, where, from the small number of impressions which Mr Roscoe could be induced to throw off, it is extremely scarce. Many of the beautiful figures in his work are from his own spirited sketches; but the majority of them are the productions of the pencil of his daughter-in-law Mrs Edward Roscoe, or of Miss R. Miller of this place.

My acquaintance with Mr Roscoe commenced in 1806, and I soon had the felicity of being received as an intimate friend. From 1810 I was further honoured by being consulted as his physician, in which capacity I watched with much anxiety over his declining health. From the time of the first derangement of the affairs of the bank, the immense mental and bodily exertions which he made produced great inroads on a constitution naturally good. He then began, on much application to any subject, to be seized with occasional faintness; and once, in 1816, he was attacked at the bank with a slight loss of memory, which speedily wore off. His habits of intense study, after this period, produced similar effects; and while engaged in the controversy on prison discipline, after writing for the greatest part of a night, to overtake a ship about to sail for America, he was affected in the winter of 1827 with partial paralysis of the muscles of the mouth and tongue. I was immediately called; the patient was freely bled—on which he recovered his speech: and the introduction of a seton in his neck removed the paralytic affection of the mouth. Intense study was forbidden: and after a period of perfect relaxation from his literary occupations, he recovered sufficiently to be able to complete his botanical work, the catalogue of Mr Coke's library, and to correct for the press his latest tracts on prison discipline. It was a great satisfaction to find his intellect quite entire, and it remained so until within an hour or two of his death. His bodily feebleness, however, gradually increased; yet, by the affectionate care of his family, his infirmities were little felt. His amusements were various reading, the illustration of his son's translation of Lanzi's History of Italian Painting, by a small collection of engravings, together with putting the last hand to his botanical work. He was unable for the fatigue of receiving much company, or of seeing strangers, for some time before his death: yet he loved to converse with a few friends, and took a lively interest in the political events with which the last year (1830) was pregnant. On the French revolution of July, he wrote a long and earnest letter to M. La Fayette (with whom he had before occasionally corresponded), urging him to use the influence of his name and popularity, to induce the French nation to spare the lives of the ministers then under arrest; pointing out how a sanguinary punishment would detract from the glory of the revolution, and what a noble opportunity the French people now had of setting an example of mitigation of the criminal code to all the nations of Europe. This letter, and another of gratulation to the present Lord Chancellor, on his attaining that high office, were the last public acts of his indefatigable and useful life *.

In the month of June 1831, he was attacked with influenza; and his exhausted frame being unable to struggle with the disease, effusion into the chest took place, and he expired on the 30th of that month, in the 78th year of his age.

Besides his published works, Mr Roscoe has left behind him a large mass of papers, and an extensive and valuable correspondence.

Among the former are various dissertations on the fine arts, some of which appear in a finished state. In the year 1814, Mr Roscoe had proposed to the writer of this memoir to undertake the translation of *Lanzi's Storia Pittorica della Italia*, and he engaged to furnish notes, and a preliminary dissertation. I had made considerable progress in the translation, when Mr Roscoe's misfortunes, and my own professional avocations, interrupted the work; which has since been well executed by his son, Mr Thomas Roscoe. Among the papers of my venerable friend, I find a very interesting introductory dissertation, intended for our joint work, tracing the history of the art of painting and sculpture to a much later period than their supposed extinction in the west, indeed almost to within 200 years of their supposed revival by the *Pisani* and *Cimabue*. This treatise is in such a state that it might be published, and it would form an excellent introduction to Lanzi's work. It is entitled, "*An Historical Sketch on the State of the Fine Arts during the Middle Ages.*"

I find also a curious dissertation on *Painters' Drawings*; another *on the Origin of Engraving on Wood and on Copper*; a third *on the Engravings of the Early German School.* There are large fragments also of a work *on the Etchings of the Italian Painters*, which contain much useful information; and a lecture "*on the use of Prints*," another on the "*Practical Part of Painting*," and two "*on the Origin and Progress of Taste.*" There is also a poem on the origin of engraving, written in 1783, of which there are two copies in MS.

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* A short while before his death, in a conversation with the writer of this [illegible]oir, he spoke calmly of his increasing feebleness, and probable early dis[illegible]ion. He "thanked the Almighty for having permitted him to pass a [illegible]f much happiness, which though somewhat checkered by vicissitude, be[illegible]hole one of great enjoyment: and he trusted that he would [illegible]lly to resign it whenever it pleased God to call him."

I have already mentioned, that the lectures on the Origin and Progress of Taste were extended into a treatise, which is not finished. This is also the case with some dissertations on the state of letters and the arts anterior to the Greeks, and their progress among that people. The whole seem to have been parts of a great work on the fine arts, which he left imperfect.

Among his papers are some MS. essays on moral and political subjects; and a considerable one "*On the principle of Vegetation and the Food of Plants.*"

His correspondence with various eminent characters is very extensive, and comprizes a period of upwards of fifty years, during a succession of most interesting events. A selection of these letters will form an appendix to the Life of Mr Roscoe, now in preparation.

The letters consist of,—

1. Correspondence on political subjects, with the Duke of Gloucester, the late and the present Lord Lansdowne, Mr Whitbread, Sir Samuel Romilly, Mr Creevy, Mr Coke, Lord Holland, the present Lord Chancellor, and President Jefferson.

2. On penal jurisprudence and prison discipline, with Mr Basil Montague, M. de La Fayette, M. Van Praet, Mr Fowell Buxton; with Dr Mease, Mr Roberts Vaux, Mr Stephen Allen, Mr Bradford, and other American gentlemen.

3. On literary subjects, with Lord Carlisle, Lord Orford, Dr Parr, Dr Symmons, Dr Aikin, Mr Samuel Rogers, Mr Thomas Campbell, Mr Montgomery, Miss Lucy Aiken, Mr Dawson Turner, Mr William Clarke, Professor William Smyth, Professor Wilson, Mr Barnard Barton, Mr Capel Lofft, Dr Channing.

4. On matters relating to the fine arts, with Sir Joshua Reynolds, Sir Thomas Lawrence, Mr Strutt, Mr Fuseli, Mr John Gibson, &c.

5. On botany and agriculture, with Sir Joseph Banks, Sir

John Sinclair, Sir James E. Smith, Dr Wallich, Dr Carey, Dr Hooker.

6. On his history of the Medici Family, with Fabbroni, Moreni, Mecherini, Professor Sprengel, Count Bossi, and Mr Henke.

In person, Mr Roscoe was tall, and rather slender. In early life he possessed much bodily activity: his hair was light auburn, almost inclining to red; his full grey eye was clear and mild; his face expressive and cheerful. As he advanced in life, the benevolent expression of his countenance remained, but the vivacity of the features was tempered into a noble dignity, which it was impossible to see without respect and admiration; while the mouth bespoke taste and feeling, and the clustering hoary hair round his temples gave a venerable air to his manly features.

There are several representations of him; but none of them appear to me so finely to express the characteristic traits of his head as John Gibson's medallion. The portrait in the Institution, and Spence's busts, give us Mr Roscoe in his decline with great fidelity. Gibson's marble bust is said to recall his youthful appearance; but the Terra Cotta medallion is Mr Roscoe, as I should wish to remember him. I may add, that it has been exceedingly well copied in the fine medal, published by Mr Clements of this town, from a die by Clint.

Of Mr Roscoe's genius and acquirements, his published works present a better memorial than any panegyric can confer; but I may be permitted to state my conviction, after having examined a great mass of his unfinished manuscript dissertations, that, had he been left to pursue his original plan of *literary retirement*, instead of again plunging into the cares and anxieties of *business*, he would have left behind a work on the History of the Fine Arts, far superior to any thing on that subject which British literature possesses.

In public life, Mr Roscoe was a consistent and fearless champion of civil and religious liberty; the uncompromising enemy of oppression, and the humane advocate of a mitigation of the [illegible]ity of penal enactments.

[illegible] the qualities of his heart, as a private individual, it is im- [illegible] to speak too highly. In the relations of husband, father, and friend, his conduct was most exemplary; and it would be difficult to point out a man who possessed the fascination of manner which attracts and rivets attachment, in a higher degree than William Roscoe.

He had deep and solemn feelings of devotion, which it was not his practice to obtrude on his acquaintances; but which he occasionally expressed to his intimate friends in the language of heartfelt piety. The beautiful invocation to the Deity, which he substituted for the intended dedication of his great botanical work, breathes the deep fervour of his adoration of the Supreme Creator; and is also remarkable as the actual suggestion of a poet's dream at the advanced age of 76.

An innate love of sincerity and truth; simplicity, combined with a playful vivacity yet suavity of manners; a generous belief in the integrity of others, the consequence of his own rectitude of purpose; an anxiety to do justice to the merits of others; a liberal and judicious patronage of modest talent, struggling to escape from obscurity, joined to a natural cheerfulness of disposition; all united to convert into devoted and enthusiastic admirers, those who first sought his friendship from his literary reputation.

The fame of Roscoe belongs to his country:—the memory of his inestimable qualities remains to his friends:

> "Multis ille bonis flebilis occidit——
> ————————cui Pudor, et Justitiæ soror
> Incorrupta Fides, nudaque Veritas,
> Quando ullum invenient parem?" Horat.

On the Mode of determining Fossil Plants. By Professor Lindley.

When a botanist proceeds to the examination of a recent specimen of an unknown plant, he directs his views to certain peculiarities in the organs, both of fructification and vegetation, *taken together*; and from what he finds to be their structure, he judges of the class, order, or genus to which it belongs. But as in fossil plants neither calyx, corolla, stamens, nor pistillum are to be recognised, an opinion has to be formed, not from the consideration of a complex combination of characters, in which the loss of one organ is compensated for by the peculiarities of those which remain; but from a few isolated and very imperfect data, exclusively afforded by the remains of the organs of vegetation. In the latter, unfortunately, the modes of organization are not sufficiently varied to enable us to draw any precise conclusions from their examination; but, on the contrary, we are often obliged to be satisfied with a general idea only of the nature of the object of our inquiry. This is, perhaps, not attended with so much practical inconvenience as might be expected, in a geological point of view, because the end of science will be sufficiently answered, if we can, in the first place, determine the general characters and affinities of the plants of the former eras; and, in the second, so exactly classify their fossil remains, as to be able to recognise them, with such precision, as to render them available for the identification of strata.

It usually happens that the only parts which are capable of being examined in a fossil state, are the internal structure of the stem, and its external surface; together with the position, division, outline, and veining of the leaves. Of these it has never yet happened that any one specimen has afforded the whole; more frequently it is only two or three of those characters that the botanist can employ.

Suppose that he has a fragment of the fossil *trunk* of some unknown tree; if no trace can be discovered of its exact anatomical structure, it may be possible, at least, to ascertain whether its wood was deposited in concentric zones, or in a confused manner; in the former case, it would have been Dicotyledonous, or Exogenous, in the latter Monocotyledonous, or Endogenous; if a transverse section should shew the remains of sinuous unconnected layers, resembling arcs with their ends directed outwards, of a solid homogeneous character, and embedded among some softer substances, then it may be considered certain that such a stem belonged to some arborescent Fern. But if the state of a fossil stem will admit of an anatomical examination, it is always desirable that it should be instituted with the assistance of the microscope. Naturalists have shewn the possibility of this being done; and if it should prove that the condition of fossil remains is in general favourable to this kind of examination, more light is likely to be thrown upon the extinct Flora than could be otherwise anticipated. If the tissue of a stem should be found entirely cellular, and it could be satisfactorly made out that no vascular tissue whatever was combined with it, the specimen would, in all probability, have belonged to that division of the vegetable kingdom, which, being propagated without the agency of sexes, is by botanists called Cryptogamia. A specimen of this kind should, however, be examined with the most rigorous accuracy; because it might have been a succulent portion of some Dicotyledonous tree, in which the vascular system was so scattered among cellular substances as to be scarcely discernible. If the tissue should have consisted of tubes placed parallel with each other, without any trace of rays passing from the centre to the circumference, it would have been Monocotyledonous or Endogenous, even if there should be an appearance of concentric circles in the wood; but if any trace whatever can be discovered of tissue crossing the longitudinal tubes at right-angles, from the centre to the circumference, then such a specimen would have been Dicotyledonous or Exogenous, whether concentric circles can be made out or not; for such an arrangement of tissue would indicate the presence of medullary rays, which are the most certain sign of a Dicotyledonous plant. If, in a specimen having these rays, the longitudinal tubes are all of the same size, a circumstance obvious upon the inspection of a transverse section, the plant will have been either Coniferous or Cycadeous; but if, among the smaller tubes, which in fact are woody fibres, some larger ones are interspersed *in a definite manner*; it would, in that case, have belonged to some other tribe of Dicotyledons. It is indispensable that the arrangement of the larger tubes should have been definite, for appearances of the same kind exist in much Coniferous wood; but, in the latter, they are scattered in an indefinite manner among the smaller tubes, and are not vessels but cylindrical cavities, for the collection of the resinous secretion peculiar to the Fir tribe. Again, if the walls of the longitudinal tubes of any fossil specimen are found to exhibit appearances of little warts, growing from their sides, such a specimen had certainly belonged to some Coniferous or Cycadeous plant, no other tribes whatever possessing such a structure at the present day. Finally, if a trace of pith can be discovered, that circumstance alone will be a proof of the plant having been Dicotyledonous, because all other classes are destitute of that central cellular column; it must, however, always be borne in mind, that absence of pith does not prove that a specimen is not Dicotyledonous, because the roots of those plants have no pith.

If a stem is in such a state that nothing can be determined concerning its anatomy, we must then proceed to judge of it by another set of characters. In the first place, it should be inquired whether it had a distinctly separable bark, or a cortical integument that differed in its organization from the wood without being separable from it; or neither the one nor the other. In the first instance, it would have been Dicotyledonous; in the second, Monocotyledonous; in the third, Acotyledonous or Cryptogamic, supposing that it had been a trunk which many successive years had contributed to form. The distinction as applied to the two latter classes, is not, however, so positive as could be wished, because tree ferns have a cortical integument, but they are easily known by the long ragged scars left by their leaves; and no other cryptogamic plants possess the character of having a spurious bark. For this reason, it is doubtful whether Calamnites is related to Equisetaceæ, and if we could be sure that the coaly matter found enveloping that genus was really the remains of a cortical integument, there would be no doubt of its affinity being of a different kind, as, for instance, with Juncus. But here is a difficulty; how are we to be sure that this coaly matter is a part of the original organization of the stem, and that it is not an independent carbonaceous formation? Another object of inquiry will be, whether the stem was articulated (as indicated by tumid *nodi*) or not; and if the former, whether it had the property of disarticulating? These circumstances are not of much positive value in pointing out affinities, but they afford negative evidence that must on no account be overlooked. For example, if this had been properly considered in regard to Calamnites, although the affinity of that genus might not have been discovered, yet it never could have been referred either to Palms or Bambusas, which, in no instance, ever disarticulate. A third and very important kind of evidence is to be collected from the scars left upon stems by the fall of leaves. Although these will neither inform us of the shape or other characters of the leaves themselves, yet they indicate, with precision, their position, the form of their base, and sometimes also their probable direction. We can tell whether they were opposite or verticillate, alternate or spirally disposed, deciduous or persistent, and imbricated or remote; all characters of great use as means of discrimination, and as often affording important negative evidence upon doubtful points. The geologist will, however, be careful not to ascribe too much value to modifications in the origin of leaves, and, in particular, to the spiral mode, which forms so striking a feature in many fossil remains: he will bear in mind, that the latter is theoretically the normal mode in which all leaves originate, and that other modes are more or less obvious modifications of it; and, finally, he will consider, that if he is not familiar with instances of it in recent plants, it is because the lines of spires are broken by the leaves that are interposed between them and the eye. He will, possibly, only remember that the leaves of firs, the fruit of the pine-apple, and the foliage of the screw pine (Pandanus), are arranged upon this plan; but, if he draw a line from base to base of the leaves of any alternate-leaved plant, always proceeding in the same direction, he will find, that that line will describe a spire round the axis from which the leaves originate; so that a spiral appearance will be apparent in proportion as leaves are approximated.

In judging of the indentity of fossil stems that are characterized by their external appearance, care must be taken not to distinguish, as different species, those stems that have still their cortical integument upon them from such as have lost it. In these two cases, the appearance of scars will be different; those of the former being more rounded, broader, and probably more deeply furrowed, than the latter; for the one is a real scar shewing the outline of the base of the leaf, while the latter is solely caused by the passage of bundles of vessels out of the stem into the petiole of the leaf.

The manner in which stems branch is sometimes well deserving consideration. Where no trace of leaves can be found, their position may possibly be indicated by the origin of branches, for the latter being always axillary to the leaves can only originate as they do: but, unfortunately, the value of this fact is often reduced to nothing by the appearance of branches from the axillæ of a few leaves only in distant parts of the stem. The most useful character to be thus derived, is when the branches regularly bifurcate, for this kind of ramifaction is a strong symptom of cryptogamic plant, especially if accompanied by an imbricated foliage.

In *Leaves*, we can rarely recognise, in a fossil state, more than their mode of *venation*, *division*, *arrangement*, and *outline*, to which are sometimes added their *texture* and *surface*. All these are of importance, but in unequal degrees. Of the highest value is the evidence afforded by the distribution of the veins, taken together with the mode of division of a leaf; if the veins are all parallel, unbranched, or only connected by little transverse bars, and the leaves undivided, the plant was probably Monocotyledonous; and if the veins of such a leaf, instead of running side by side from the base to the apex, diverge from the midrib, and lose themselves in the margin, forming a close series of double curves, the plant was certainly analogous to what are now called Scitamineæ, Marantaceæ, and Musaceæ; but, supposing that the parallel arrangement of simple veins is combined with a pinnated foliage, then the plant would probably have belonged to Cycadeæ, that curious tribe that stands on the very limits of Monocotyledons and Dicotyledons, and of flowering and flowerless plants. By such characters as these however, there is no means of distinguishing certain palms, if in a fossil state, from Cycadeæ.

If veins are all of equal thickness, and dichotomous, we have an indication of the Fern tribe, which is seldom deceptive. Nevertheless, it must be remembered that the flabelliform leaves, both of Monocotyledons and Dicotyledons, have occasionally this kind of variation. Even if the veins are not dichotomous, if they are all of nearly equal thickness and very fine, or divided in a very simple manner, it is probable that they indicate the Fern tribe, whether simple, as in the fossil genus Taeniopteris, or reticulated, as in the modern genus Meniscium. If veins are of obviously unequal thickness, and so branched as to resemble the meshes of a net, we have a sign of Dicotyledonous structure that seldom misleads us.

Finally, if no veins at all are to be found, an opinion must *be formed*, not from their absence but from other circumstances.

If the leaves are small, their absence may be due to incomplete development; but if the leaves are large and irregularly divided, we may have an indication of some kind of marine plant. When leaves are small and are densely imbricated, they are generally considered by fossil botanists to belong to either Lycopodiaceæ, or Coniferæ; and there is so little to distinguish those families in a fossil state, that there is scarcely any means of demonstrating to which such genera as Lycopodites, Lepidodendron, Juniperites, Taxites, and the like actually belong.

It would be easy to extend these observations much further, but to dwell at length upon this branch of the subject, would carry us far beyond our present limits. We will, therefore, bring our remarks to a conclusion, by calling attention to some of those points, to the elucidation of which, it is most to be wished, that geologists, who have opportunities of collecting fossil plants, would apply themselves.

In the first place, evidence is wanted as to plants to which the cones called Lepidostrobi, the leaves called Lepidophylla, and the fruit named Cardiocarpa, respectively appertain. Are they all portions of species of the same genus, or, as seems more probable, is not Cardiocarpon a part of a plant of a totally different affinity? Secondly, what were the leaves of Sigillaria and of Stigmaria? Of the latter, something is known; but the leaves are always so crushed, that no notion can be formed of their exact nature. Mr Steinhauer says he has traced them to the length of 20 feet! In the third place, to determine the leaves of any of the fossil stems that at present are only in the latter state, such as Sternbergia, Bucklandia, Cycadeoidea, Caulopteris, Exogenites and Endogenites, would be to supply a great desideratum. Again, what was the real nature of the stem of Calamites? Was it an annual shoot, proceeding from a perennial horizontal rhizoma, like that of Juncus, &c.? Had it any leaves, and if so, were they of the nature of those figured in our work, as probably belonging to Calamnites nodosus, but considered by Sternberg and Brongniart a distinct genus, which they call Volkmannia? Another very interesting object of inquiry is into the anatomical structure of Lepidodendron, for the sake of settling whether that extensive fossil genus belonged to Coniferæ or to Lycopodiaceæ, or to neither. We know nothing of the leaves belonging to the fossil fruits called Anomocarpon Musocarpon, &c., or of the fruit of Cycadeoidea, Annularia, Asterophylites and many others. Now these are difficulties that probably may be removed with diligent research among the beds in which such fossils occur; and which, if removed, would contribute much more to fixing the science upon a solid basis, than the discovery of species not before described.—*Lindley and Hutton's Fossil Flora of Great Britain.*

On the Vitality of Toads. By the Rev. EDWARD STANLEY, F. L. S., F. G. S., &c. Communicated by the Author.

SIR,

IN No. 25 of your valuable Journal, page 26, you have inserted an interesting paper on the vitality of toads by Professor Buckland. If other experiments, partly similar, attended with similar results, are worthy of recording, you may insert the following.

On the 22d June 1830, I placed three toads, No. 1, weighing 6 drachms; No. 2, weighing 17½ drachms; No. 3, weighing 31½ drachms, in separate flower-pots, covering the surface with a tile, as well as the hole at the bottom. They were then buried about four feet deep in garden soil, and dug up on the 21st March 1831, all dead. It appeared, however, from the state of their remains, that they had died at different times, the body of one being in nearly a perfect state, whereas of another nothing remained but the bones. Conceiving that the damp state of the soil might have affected them, I placed a 4th in a wide mouthed bottle, corked down, in a dry place, inserted a quill through the cork for the admission of air, and a 5th in a similar bottle, well corked, entirely excluding air, which died before I was aware of it, in 48 hours *; proving very decidedly that air is as essential to their existence as to any other class of living animals. No. 4. continued in apparently good health for about a fortnight: it then, however, began to shew signs of weakness, and its eyes became dim, in fact, it appeared so nearly exhausted and dying, that I released it from its confinement, and placed it under a flower-pot on moist garden earth, where it could meet with worms and small insects, and enjoy a sufficiency of air. The consequence was, that in the course of a day or two it had completely recovered its natural colour, brilliancy of eye, and full motion of its limbs; and, when finally released, I had the pleasure of seeing it crawl off under every symptom of entire convalescence. One fact I remarked worthy of notice. I have mentioned that Nos. 4 and 5 were placed in bottles in a dry situation. In consequence probably of this, at the end of about 12 hours I observed them to be in a state of violent perspiration as if every pore was exuding moisture, to such a degree, indeed, that the sides of the glass were covered with a strong dew, which accumulated till it formed drops, collecting at the bottom to the amount of about one-fourth of a teaspoonful. Of the precise nature of this liquid, I am ignorant, but it probably contained a portion of some powerful acid, as in a very short time I observed the ink to disappear from the small labels I had enclosed in the bottles, containing the weights of the toads and dates of their confinement. It is clear from these several experiments, that the commonly received belief that these reptiles can exist in blocks of stone and stems of solid wood, is perfectly false; notwithstanding the almost numberless instances on record, apparently well attested, of their vitality under the joint additional singularity of exclusion of air and privation of food. My experiments are, indeed, more to the point of their inability to exist for any length of time unless amply supplied with air and food of their own selection, than Dr Buckland's; for I should observe, that although in the instance of No. 5, absolute exclusion of air, exclusion of food is also implied, in the cases No. 1, 2, 3, both food and air were both more or less amply supplied, for, in the flower-pots I not only found several small scolopendra, but quantities of more minute insects, consisting chiefly of the *Podura fimetaria.* Now, it is evident, that to that depth, air must have penetrated, or the insects themselves could not have existed; and that it must have been to a degree quite sufficient for animal life, is equally certain from these insects having descended so far from choice. *There is still, however,* considerable mystery in the history of these creatures remaining to be cleared up, and so much conflicting evidence, that I cannot bring my mind to any very positive conclusions *. I certainly have never been fortunate enough to meet with one of the many instances of toads said to have been found hermetically immured in wood or stone, where they must have existed for several years at least; but I have met with them in situations, such as bottoms of shafts, caverns, cellars and crevices, where I could not easily account for a due supply of their known usual food, and, in fact, I selected the imprisonment under flower-pots, as the nearest mode of combining their natural habits with the supposed theory of existence under absolute exclusion. That No. 5, placed in the bottle, died from exclusion of air I think there can be no doubt; and that No. 4, though supplied with air, would have died in another 24 hours, is most probable; but I think it not impossible but that some other causes might have operated, for whenever I observed them, I could not perceive the slightest appearance of uneasiness or sense of restraint; they appeared exactly according to their usual habits, in a sort of dull dormant quiescent state, just as when found in some secluded chink in rocks or under stones, when the only predominant feeling appear to be dissatisfaction at any interruption to their sluggish life, accompanied by an unnatural excitement (I will not call it activity) of their limbs, occasioned by fear, and anxiety to make them retreat with all speed to some similar abode under some other "cold stone," wherein again to resume their "iron sleep." In a word, the awkward dull movements and lethargic eye of a disturbed toad, seem to express the very sentiments of the Scandinavian prophetess to Odin:

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* It is scarcely necessary to add, that the air in the bottle was totally unfitted for respiration or combustion; a lighted taper plunged into it was immediately extinguished.

> "Now my weary eyes I close,
> Leave me, leave me, to repose."

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* As the experiments of Herissant in 1777, and Dr Edwards in 1817, may not be generally known, I shall briefly state, that the former shut up three toads in sealed boxes in plaster, which, after having been deposited for eighteen months in the Academy of Sciences, were opened and two of the toads were found to be living, one alone having died. Dr Edwards enclosed some toads totally in plaster, and absolutely, as far as he could, deprived them of air: they all lived many days, and much longer than those which were forced to remain under water. This singular result can only be accounted for on the supposition that air must have penetrated through the plaster.

On a Production of Naphthaline in an Oil-Gas Apparatus. By A. Connell, Esq. F. R. S. E. Communicated by the Author.

Some white crystalline matter, said to have been deposited by oil-gas, was lately given me by Professor Jameson for chemical examination. This matter was crystallized in thin tables adhering together in groups, and possessing a fine pearly lustre. A partial discoloration was occasioned by the intermixture of a little impurity, consisting of oxide of iron, with some adhering carbonaceous matter, which gave an empyreumatic smell when heated. The white crystals were found to possess the well-known chemical properties of naphthaline; and I should not have thought it necessary to take any farther notice of them, had it not been that the source from which they proceeded, and the circumstances under which they were formed, were somewhat peculiar, and may afford some illustration of the conditions necessary to the production of this body.

Upon making inquiry at the place where the crystals were produced, I was informed that they were first observed in an old iron pipe several feet in length, and a few inches in internal diameter, which had five years before formed part of a private oil-gas apparatus. This pipe had been situated between the retort on the one hand, and the condenser and gasometer on the other; and when the apparatus was in operation, an empyreumatic oil used to be deposited in it, but no formation of the white crystals had ever been observed during that period. The material which had been employed for the production of the gas was at first whale-oil; then palm-oil had been used; and latterly whale-oil had been again employed. Some of the crystals which I saw taken out of the pipe were mixed with a black soft pitchy-looking matter, which seemed to line its inside.

When this dark matter was heated on platinum foil over the spirit-lamp, it fused at a moderate heat, and gave off dense white vapours. When the flame of the lamp was brought in contact with it, it took fire and burned away, leaving a considerable residue, which was attracted by the magnet, and was iron more or less oxidized. When some of the dark matter was heated in a tube, it gave off at a moderate temperature a little naphthaline, which condensed in the upper part of the tube, and as the heat was increased, a yellow fluid condensed on the sides of the tube, which was to a great extent soluble in alcohol.

Several views may be taken of the mode of production of the naphthaline in the gas apparatus. We might suppose that it had been formed during the destructive distillation of one or both of the oils employed and deposited in crystals at the time of its formation; but this view is opposed by the circumstance, that no naphthaline was observed whilst the gas apparatus was in operation; and it is also improbable, that crystals of the size obtained and so well defined, should have remained during so long a period in a situation to which there was access of air, when the great volatility of naphthaline is considered. We may conceive, in the next place, that the naphthaline, after it had been formed by a new arrangement of elements during the destructive distillation of the oil, was condensed along with some of the other products and held dissolved by them, until the solvent was decomposed or dissipated by the slow operation of time and the external air. Or, lastly, we may suppose that it was not formed during the distillation, but first existed only through the slow and spontaneous decomposition of some of those products, with access of air, after the apparatus had been laid aside. The last view is perhaps on the whole the most probable.

The influence of external air in the production, or at least development, of naphthaline, has been lately observed by M. Laurent, who found that naphthaline was not obtained in all cases by the distillation of coal tar, and that its production was most certain when the coal tar had been long exposed to the air *. M. Reichenbach did not succeed at all in getting naphthaline by the distillation of coal tar, but the tar employed by him had been apparently recently prepared †. M. Laurent also found that the action of atmospheric air was superseded by that of chlorine.

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* Annales de Chimie et de Physique, xlix. 220.

† Ib. xlix. 36.

On the Characters and Affinities of certain Genera, chiefly belonging to the Flora Peruviana.

By Mr DAVID DON, Librarian of the Linnean Society; Member of the Imperial Academy Naturæ Curiosorum; of the Imperial Society of Naturalists of Moscow; of the Royal Botanical Society of Ratisbon; and of the Wernerian Society of Edinburgh, &c. (Continued from No. for Oct. 1831, p. 280.)

ZIGADENUS, *Mich.*

Syst. Linn. HEXANDRIA TRIGYNIA.

Ord. Nat. MELANTHACEÆ, *Br.*

Perianthium 6-partitum: *segmentis* lanceolatis, acuminatis, basi biglandulosis. *Filamenta* subulata, basi dilatata. *Antheræ* reniformes, biloculares: *loculis* confluentibus. *Ovarium* (e folliculis 3 connatis compositum) triloculare: *ovulis* indefinitis. *Stigmata* simplicia, canaliculata, pruinosa, recurvata. *Capsula* membranacea, trigona, 3-locularis: *loculis* di-polyspermis. *Semina* adscendentia, complanata, basi parùm alata: *testâ* membranaceâ, laxâ, nucleo ejusdem majori.

Herbæ (Amer. Septentr.) *caulibus ramosis.* Folia *lineari-lanceolata, acuminata, nervosa, marginibus carinâque scabris.* Racemus *divisus, paniculatus, bracteatus.*

1. *Z. frigidus*, ovario semiinfero! loculis polyspermis, stylis solutis abbreviatis.

Veratrum frigidum, *Deppe et Schiede in Schlecht. Linnæâ*, 6. p. 46.
Ytzcuimpatli tertia, *Hernand. Mex.* p. 307.

Hab. In pratis elevatis Orizabæ montis, alt. 10,000 ped.—*Schiede et Deppe.* ♃. (V. s. sp. in Herb. Lamb.)

Rhizoma bulbo-tuber ovato-oblongum, foliorum emarcidorum rudimentis vestitum. *Caulis* erectus, ramosus, teres, lævis, viridis, ferè omninò solidus, bi- v. tripedalis, calami scriptorii crassitie. *Folia radicalia* linearia, acuta, conduplicata, glabra, margine membranacea, minutissimè crenulata, recurvato-patentia, nervis plurimis parallelis striata, pedalia v. ultrà, basi valdè dilatata; *caulina* breviora, erecta; *superiora* adpressa. *Racemus* elongatus, paniculatus. *Flores* albi. *Bracteæ* lanceolatæ, acuminatæ, membranaceæ, basi cucullatâ pedunculos vaginantes, iisdemque longiores.

Perianthium 6-partitum, basi turbinatâ adhærens: *laciniis* lanceolatis, acutis, æqualibus. *Stamina* 6, perianthii laciniis opposita: *filamenta* subulata, complanata, glabra, basi dilatata atque in annulum brevissimum connata: *antheræ* reniformes, biloculares: *loculis* confluentibus, rimâ undique dehiscentibus. *Ovarium* semi-inferum! e folliculis 3 conflatum: *ovulis* indefinitis. *Styli* 3, compressi, distincti, recurvati, glabri, breves. *Stigmata* cucullata, pruinosa. *Capsula* ovata, membranacea, 3-locularis: *loculis* polyspermis. *Semina* compressa, scobiformia, margine altero truncato: *testâ* laxâ, tenuissimè membranaceâ.

This genus is so closely allied to the following, that, besides the glands at the base of the segments of the perianthium, and the branched inflorescence, there is little else to separate them. The species is well distinguished by its half-inferior ovarium, and by the shorter and unconnected styles.

HELONIAS, *Mich.*

HELONIÆ sp. *Linn.*

VERATRI sp. *Auct.*

Syst. Linn. HEXANDRIA TRIGYNIA.

Ord. Nat. MELANTHACEÆ, *Br.*

Perianthium 6-partitum: *segmentis* obtusis, subæqualibus. *Stamina* 6: *filamenta* subulata, basi parùm dilatata: *antheræ* reniformes: *loculis* confluentibus. *Stigmata* subulata. *Capsula* 3-locularis: *loculis* oligospermis. *Semina* compressa: *testâ* membranaceâ.

Herbæ (Amer. Bor.) *paludosæ.* Scapi *simplicissimi.* Racemus *spicatus, bracteatus.* Capsula *membranacea.*

OBS. In Xerophyllo antheræ loculis distinctis, filamenta basi sæpè dilatata, perianthii segmenta 3 interiora latiora, racemus corymbosus bracteatus, caulis foliosus.

In Heloniâ luteâ, quæ rectè in novum genus constituit ill. Decandolle, flores sunt dioici, filamenta et stigmata clavata, antheræ oblongæ, apice emarginatæ, loculis parallelis distinctis, nec confluentibus, racemus ebracteatus, caulis foliosus.

1. H. *officinalis*, foliis linearibus prælongis carinatis margine scabris, racemo elongato, seminibus compressis apice alatis.

Veratrum officinale, *Schlecht. in Linnæâ*, 6. p. 45.

Ytzcuimpatli secunda, *Hernand. Mex.* p. 307.

Hab. In graminosis ad latera montium Hacienda de Laguna et Barranca de Tiorelo in provinciæ Veræ Crucis regionibus temperatis. *Schiede et Deppe.* ♃. (V. s. sp. in Herb. Lamb.)

Radix crassa, subbulbosa, fibras complures longissimas basi emittens: *collo* elongato, rudimentis foliorum emarcidorum fibrosis in circulis concentricis dispositis obvoluto. *Scapus* e foliorum medio solitarius, iisque multò altior, erectus, teres, lævis, solidus, medullâ spongiosâ farctus, pennæ corvinæ crassitie. *Folia* plurima, longissima, recurvato-patentia, linearia, acuta, carinata, margine denticulis cartilagineis serrulato-scabra, nervis plurimis parallelis prominulis intermedioque validiori peragrata, luteo-viridia, tactu aridissima, sesqui v. tripedalia, 3 lineas lata. *Racemus* spicatus, terminalis, erectus, 4-5-uncialis; *fructiferus* prolongatus, pedalis et ultra. *Flores* parvi, albi, exsiccatione lutescentes, undique versi, conferti, erecti. *Pedunculi* teretes, validi, læves, brevissimi. *Bracteolæ* recurvatæ, truncatæ, margine scariosæ, laceræque, persistentes. *Perianthium* marcescens, 6-partitum: *segmentis* simplici ordine æqualibus, linearibus, apice crassiore obtusis, basi disco parùm elevato calloso auctis. *Stamina* 6, segmentis perianthii opposita, æqualia: *filamenta* subulata, glabra, persistentia, basi parùm dilatata: *antheræ* reniformes, medio filamentis adnatæ: *loculis* apice confluentibus; suturâ undique dehiscentibus: *valvulis* cartilagineis. *Pollen* pulvereum. *Ovarium* (è folliculis 3 compositum) superum, 3-loculare, perianthio filamentisque marcescentibus basi cinctum: *ovulis* in quoque loculo pluribus (6) erectis, suturæ internæ insertis; *imis* 2 v. 3 tantùm maturescentibus! *Stigmata* 3, subulata, acuta, recurvata, pruinosa. *Capsula* ovato-oblonga, 3-locularis, 3-valvis: *loculis* 2-spermis. *Dissepimenta* e marginibus valvularum introflexis constituta. *Semina* compressa, apice complanato-alata: *testâ* membranaceâ, atrofuscâ, nitidâ, albumini arctè adhærenti: *albumen* copiosum, cartilagineum, album. *Embryo* parvus, teres, in basi albuminis erectus, concolor.

There is, I think, much reason for believing this to be the second kind of the *Ytzcuimpatli* referred to by Hernandez, for it can scarcely be the same with his first sort, if the figure and description of the leaves can be relied on. Whether this last is identical with the plant figured by Descourtilz, or which of them is actually the *Cevadilla* of Monardes, are questions which I have not materials to decide.

As all the plants of this family possess nearly the same properties, it is not improbable that different species have been applied in different countries to the like purpose. The *Sabadilla* or *Cevadilla* formerly held an important place in the Pharmacopœia, but happily the progressive habits of cleanliness in the people has rendered it now no longer necessary.

XIPHIDIUM, *Læfl.*

Syst. Linn. TRIANDRIA MONOGYNIÆ.

Ord. Nat. HÆMADORACEÆ, *Br.*

Perianthium 6-phyllum; *foliolis* 3 *interioribus* paullò majoribus. *Stamina* 3, his opposita; *tertio* majore, filamento triplò latiore, complanato. *Antheræ* anticæ, biloculares, erectæ: *loculis* distinctis parallelis, saturâ longitudinali dehiscentibus. *Ovarium* globosum, 3-loculare, omninò liberum: *ovulis* numerosis, peltatis, processibus placentæ adnatis. *Stylus* indivisus. *Stigma* simplex, truncatum, papillosum. *Bacca* sphærica, 3-locularis: *loculis* polyspermis. *Dissepimenta* simplicia, membranacea. *Placentæ* 3, angulo loculorum interiori insertæ, crassæ, succulentæ, processibus elevatis undique tuberculatæ. *Semina* depresso-angulata, apice disco dilatato concaviusculo: *testa* cartilaginea: *albumen* farinaceum, niveum. *Embryo* minutus.

Herbæ (Amer. Merid.) Veratri *facie*. Radix *fibrosa, repens*. Folia *alterna, ensiformia, striato-nervosa, equitantia*. Flores *terminales, paniculati, parvi*. Bracteolæ *scariosæ, brevissimæ*. Bacca *violacea*.

1. X. *rubrum*, foliis erectis margine denticulatis, paniculâ racemosâ, perianthii foliolis ovato-oblongis obtusis, antheris ovalibus.

Ornithogalum rubrum. *Ruiz et Pavon MSS.*

Hab. in Peruviâ. *Ruiz et Pavon.* ♃. (V. s. sp. in Herb. Lamb.)

Notwithstanding its free ovarium, this genus evidently belongs to the *Hæmodoraceæ*, to which it had been doubtfully referred by the distinguished author of the *Prodromus Floræ Novæ Hollandiæ*, to whom the structure of its fruit appears to have been then unknown. The ovarium is also free in *Wachendorfia*, so that even among Monocotyledonous plants this character is not of primary value.

ZEPHYRA.

Syst. Linn. TETRANDRIA MONOGYNIA!

Ord. Nat. ASPHODELEÆ, *Br.* *Trib.* 3. CONANTHEREÆ, *Nobis.*

Perianthium semisuperum, monophyllum, subhypocrateriforme, coloratum, deciduum: *tubo* cylindraceo, basi circumcissâ, parùm dilatatâ: *limbo* 6-partito, patenti: *segmentis* elliptico-oblongis, 7-nerviis (nervis plerumque ramosis); *exterioribus* paullò angustioribus, mucronulo tuberculiformi papilloso apiculatis. *Stamina* 6, fauci perianthii inserta; 2 longiora, sterilia! *filamenta* compressa, glabra: *antheræ* ovato-oblongæ, apice obtusæ, inappendiculatæ, valvulis licet longitudinaliter solutis e foraminibus 2 terminalibus tantùm pollen effundentibus; *loculo altero* (interiore) basi in corniculum brevissimum producto. *Ovarium* 3-loculare: *ovulis* pluribus. *Stylus* tenuis. *Stigma* punctum pruinosum. Capsulam maturam nondùm vidi.

Herba (Peruviana) *elegans.* Rhizoma *bulbo-tuber?* Caules *erecti, ramosi, glabri, nudiusculi, basi rudimentis scariosis membranaceis foliorum imperfectè evolutorum obvoluti, palmares.* Folia radicalia *mihi ignota;* caulina *paucissima, rudimentaria, lineari-lanceolata, acuminata, canaliculata, nervosa, glabra, plerumque adpressa, vix uncialia.* Flores *numerosi, paniculati, azurei.* Pedicelli *simplicissimi, filiformes, summo apice dilatato cum perianthio articulati.* Bracteolæ *subulatæ, virides, pedicellis breviores.* Perianthium 10 *lineas longum.*

1. *Z.* elegans.

Hab. In Peruviæ provinciâ australiori Tarapacensi ad Lomas de Iquique. *D. Bollaert.* ♃. *Vulgò* Flor de Viuda. (V. s. sp. in Herb. Lamb.)

This interesting genus accords entirely in habit and in the structure of its perianthium with *Cummingia,* from which it is, however, essentially distinguished by the circumstance of two of the stamens being sterile, and the absence of the membranous points from the anthers, one of the cells of which is developed at the base into a short spur-like process. The specimens whence the above description was derived, formed part of a small but valuable collection of plants made by Mr Bollaert, formerly chemical assistant at the Royal Institution, in the course of his travels in the interior of Peru. The singular circumstance of two of the stamens being sterile, leaving four fertile, is worthy of particular notice, as forming a remarkable exception to the law of development of organs among the *Monocotyledoneæ.*

PASITHEA.

Syst. Linn. HEXANDRIA MONOGYNIA.

Ord. Nat. ASPHODELAÆ, *Br.* *Trib.* 3. CONANTHEREÆ, *Nobis.*

Perianthium omninò superum! profundè 6-partitum: *foliolis* inæquilateri-oblongis, mucronulo subcucullato, papilloso apiculatis, 3-nerviis; *interioribus* latioribus. *Stamina* 6, basi corollæ inserta: *filamenta* subulata, complanata, glabra: *antheræ* lineares, incumbentes! biloculares: *loculis* paral-

lelis, basi solutis, longitudinaliter dehiscentibus. *Ovarium* 3-loculare: *ovulis* indefinitis, *Stylus* filiformis, glaber. *Stigma* trifidum: *laciniis* recurvis, obtusis, pruinosis. *Capsula*..........

Herba (Chilensis) *elegans*. Radix *fasciculato-fibrosa*. Caulis *erectus, ramosus, teres, glaber, bi-v. tripedalis*. Folia *radicalia, caulis fere longitudine, basibus membranaceis equitanti-imbricatis disticha, recurvato-patula, linearia, acuminata, plana, nervosa, graminea, submembranacea, nervo intermedio magis prominulo, suprà basin versus lineâ dilatatâ albâ notata, margine cartilagineo, lævi; semipollicem v. pollicem lata;* caulina *lineari-lanceolata, acuminata, subadpressa, 2-4-uncialia*. Flores *diffusè paniculati, cyanei*. Pedicelli *filiformes, inarticulati, summo apice dilatati*. Bracteolæ *lanceolatæ, acuminatæ, brevissimæ, membranaceæ*. Perianthium *basi circumcissâ deciduum*. Ovarium *apici pedicelli ferè immersum*.

1. P. cœrulea.

Anthericum cœruleum, *Ruiz et Pavon Fl. Peruv. et Chil*. 3. p. 67, t. 299. f. b.

Bermudiana cœrulea, Phalangii ramosi facie, vulgò *Illeu*. *Feuill. Peruv.* 1. p. 712. t. 8.

Hab. in Chili locis umbrosis. *Feuillée, Ruiz et Pavon, Cuming.* ♃ *Vulgò* Illeu. (V. s. sp. in Herb. Lamb.)

Besides the present, several other plants have been referred to *Anthericum* by the authors of the *Flora Peruviana*, which clearly do not belong to that genus; and among them their *Anthericum plumosum* and *coarctatum* are deserving of particular notice, as constituting the rudiments of two new genera apparently belonging to the normal group of *Asphodeleæ*. The inferior or half-inferior ovarium, and the simple, inarticulate pedicels, have induced me to regard the *Conanthereæ* as forming a separate section of the order. The incumbent anthers and the trifid stigma essentially distinguish *Pasithea*, both from *Conanthera* and *Cummingia*.

It cannot fail to be remarked, that the triple stigma and the completely inferior ovarium bring this genus very near to the *Hypoxideæ*, affording one among many other proofs that might be adduced of the extreme difficulty, or rather utter impossibility, of establishing absolute characters in Natural History.

ERCILLA, *Ad. Juss.*

Syst. Linn. DECANDRIA PENTAGYNIA.

Ord. Nat. PHYTOLACEÆ, *Nobis.*

Calyx 5-phyllus, basi 3-bracteolatus: *foliolis* ovalibus, concavis, membranaceis. *Petala* 0. *Stamina* 10, perigyna; *alterna* calycis foliolis opposita: *filamenta* capillaria, glabra: *antheræ* incumbentes, biloculares: *loculis* parallelis, distinctis, mediante apice filamentorum connatis, utrâque extremitate solutis, obtusis. *Ovarium* breviter pedicellatum, 6-gonum, 6-loculare. *Styli* 6, subulati. *Stigmata* totidem, simplicia, obtusa, recurvata, pruinosa. *Ovula* solitaria, erecta, rapheos basi adnata. Bacca (maturam nondùm vidi) 6–cocca: *coccis* monospermis, angulo interiori adnatis, exteriore longitudinaliter dehiscentibus.

Frutex (Chilensis) *volubilis, glaberrimus.* Folia *sparsa, petiolata, ovalia, obtusa, coriacea, margine cartilagineo-integerrima, unicostata, venis immersis, 2–3-uncialia.* Racemi *axillares ! solitarii, subspicuti, penduli, bipollicares.* Flores *albi.* Bracteolæ *elliptico-oblongæ, concavæ, membranaceæ.*

Obs. A Phytolaccâ tantùm differt ovario pedicellato, et inflorescentiâ axillari, nec oppositifoliâ.

1. Ercilla volubilis, *Ad. de Juss. in Ann. des Scien. Nat.* 25. p. 11. t. 3. f. 1.
Suriana volubilis, *Dombey et Cav. MS.*
Lardizabala sp. nova. *Ruiz et Pavon MS.*

Hab. in Chili ad Valparaiso. *Ruiz et Pavon, Dombey, Lud. Née, Bridges, Cuming.* ♄ ◡. (V. s. sp. in Herb. Lamb.)

The intimate affinity of this genus to *Phytolacca* leaves no doubt of its being a legitimate member of the same natural family, notwithstanding the opinion to the contrary held by M. Adrien de Jussieu, who, in the memoir above mentioned, has proposed to refer it to the *Menispermeæ*, from which, it appears to me, the genus differs widely, especially in the anthers being incumbent with distinct parallel cells, in its long filiform stigmata, in the structure of the ovaria, and in the leaves being furnished with a solitary midrib, with transverse branches. I had already described the genus, and referred it to *Phytolaceæ*, when the learned memoir of M. Adrien de Jussieu fell into my hands. The twining habit and the axillary inflorescence are the only characters with which it agrees with the *Menispermeæ;* but these are mere points of analogy, and I am persuaded that there exists no affinity whatever between *Ercilla* and that family. The habit of the plant appears to have misled Ruiz also in regard to its affinity.

ANISOMERIA.

Syst. Linn. POLYANDRIA PENTAGYNIA.

Ord. Nat. PHYTOLACEÆ, *Br.*

Calyx 5-fidum: *laciniis* inæqualibus, rotundatis, subcoriaceis; 3 *superioribus* majoribus. *Petala* 0. *Stamina* plurima (20–30), basi perianthii inserta, ad latus superius assurgentia, subsecunda: *filamenta* filiformia, glabra, persistentia: *antheræ* incumbentes, biloculares: *loculis* distinctis, parallelis, longitudinaliter dehiscentibus. *Pistilla* plura (4, 5 v. 6) distincta, axi destituta. *Ovaria* ancipiti-compressa. *Stigmata* recurvata, longitudinaliter papillosa. *Baccæ* abortu 2, 3 v. rariùs 5 v. solitariæ, monospermæ.

Herba *perennis.* Caules *plures, simplices, erecti, foliosi, semipedales, digiti minoris crassitie.* Folia *alterna, sessilia, ovato-oblonga, apice mucronulato, recurvo.*

Spica *terminalis, solitarius, peduncularis, pedalis.* Flores *sparsi, rariùs gemini, sessiles, cernui, bracteolâ spathulatâ, membranaceâ suffulti, virides.*

1. A coriacea.
Hab. in Chili ad Valparaiso. *Cuming.* ♃. (V. s. sp. in Herb. Lamb.)

This ought perhaps to be considered as merely a species of *Phytolacca*, rather than as a distinct genus. It differs from *Phytolacca* chiefly in the inequality of the lobes of the calyx, the ascending stamina, and the absence of an elevated central axis, leaving the ovaria entirely free at their inner edge.

MACROMERIA.

Syst. Linn. PENTANDRIA MONOGYNIA.

Ord. Nat. BORAGINEÆ, *Juss.* *Trib.* 3. Lithospermeæ, *Nobis.*

Calyx altè 5-partitus, parùm inæqualis. *Corolla* infundibuliformis, calyce multò longior, limbo 5-lobo: *lobis* acutiusculis, erectis. *Stamina* 5, subæqualia: *filamenta* capillaria, glabra: *antheræ* lineari-oblongæ, biloculares, incumbentes, versatiles: *loculis* parallelis, longitudinaliter dehiscentibus. *Stylus* capillaris, glaber. *Stigma* punctum pruinosum. *Ovaria* 4, connata.

Herbæ (Mexicanæ) *perennes.* Caules *erecti, simplicissimi.* Folia *alterna, sessilia, lanceolata, nervosa.* Racemi *pauciflori, bracteati.* Calycis segmentis *linearibus, erectis, parùm inæqualibus.* Corolla *alba? calyce triplò v. 5-plò longior, 2-3-pollicaris:* tubus *basi attenuatus:* faux *dilatata:* limbus 5-*lobus:* lobis *oblongis, erectis, æstivatione invicem se imbricatis.*

1. M. *longiflora*, foliis lanceolatis acuminatis læviusculis, genitalibus inclusis.
Lithospermum longiflorum. *Herb. Sesse et Mocinno.*
Hab. in Mexico. *Sesse et Mocinno.* ♃ (v. s. sp. in Herb. Lamb.)

2. M. *exserta*, foliis lanceolatis mucronatis scabris, genitalibus longè exsertis, caule hispido.
Echium sp. *Herb. Sesse et Mocinno.*
Hab. in Mexico. *Sesse et Mocinno.* ♃ (v. s. sp. in Herb. Lamb.)

The flowers are by far the largest of the whole family, of which it may justly be reckoned the most showy genus. The long filaments essentially distinguish it from *Lithospermum* and *Batschia*, to which it otherwise comes nearest in affinity. The *Boragineæ* may be divided into five very natural groups, distinguished by the form and structure of their corolla. The *first*, consisting of *Borago*, *Trachystemon* *, *Trichodesma*, and perhaps also *Rindera*, characterised by a deeply-parted corolla, with pointed segments, exserted stamina, and a diffuse inflorescence, may be termed *Borageæ*. The *second* may be denominated *Symphyteæ*, comprehending *Symphytum*, *Onosma*, *Onosmodium*, *Cerinthe*, *Mertensia* and *Pulmonaria*, having a tubular, truncate corolla, with very short lobes, and the inflorescence rarely revolute. The *third* are the *Lithospermeæ*, having an open tubular corolla, with broad, mostly rounded lobes, and the stamina often exserted, consisting of *Lithospermum*, *Messerschmidia*, *Batschia*, the present genus, *Moltkia*, *Echium* and *Lycopsis*. The *fourth* are the *Buglosseæ*, having a salver-shaped corolla, with the mouth closed with vaulted processes, comprising *Anchusa*, *Myosotis*, *Omphalodes*, *Cynoglossum*, and *Asperugo*. The *fifth* are the *Heliotropeæ*, consisting of *Heliotropium*, *Tournefortia*, and *Tiaridium*, agreeing in the form of their flower, with the last group, but remarkable for the plicate limb of their corolla, and for their connate fruit, approaching the simple drupe of *Cordiaceæ*, to which they may very properly be considered as forming the transition. To the latter family belongs *Cortesia* of Cavanilles, which does not seem to be generically different from *Beurreria* of Jacquin. The mode of inflorescence in *Cordiaceæ*, their parted style, and the nature of their albumen, connects them on the one hand with *Convolvulaceæ*, and on the other with *Hydroleaceæ*, and from this last to the *Polemoniaceæ* the transition is pretty evident.

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* This genus is founded on *Borago orientalis*, which differs essentially from *Borago*, by its lengthened hairy filaments; by its incumbent, pointless anthers; and lastly by the blunt abbreviated processes of the corolla.

NIEREMBERGIA, *Ruiz et Pavon*.

PETUNIA, *Juss.*

NICOTIANÆ, sp. *Auct.*

Syst. Linn. PENTANDRIA MONOGYNIA.

Ord. Nat. SOLANEÆ, *Br.*

Calyx profundè 5-fidus: *lobis* ligulatis, foliaceis. *Corolla* hypocrateriformis: *fauce* nunc ventricosâ: *limbo* patenti, 5-lobo, 5-plicato. *Stamina* 5, tubo inserta, parùm inæqualia: *filamenta* capillaria, glabra, infernè adnata: *antheræ* subrotundæ, bilobæ, biloculares: *loculis* basi parùm divergentibus, apice distinctis, rimâ longitudinali exteriùs dehiscentibus. *Ovarium* biloculare, basi disco cyathiformi cinctum. *Stylus* ancipiti-compressus, glaber, apice dilatatus. *Stigma* reniforme, compressum, v. capitatum, viscidum, nunc minutissimè papillosum: *lobis* connato-applicatis, margine revolutis, subinde quasi sulco medio exaratis. *Capsula* ovata, bilocularis: *valvulis* bifidis. *Dissepimentum* contrarium (facie ad caulem), undique rumpens, tandem solutum. *Placentæ* 2, septo adnatæ. *Semina* subreniformia: *testa* crustaceâ, scrobiculata: *albumen* carnosum. *Embryo* teres, arcuatus, albus: *cotyledones* lineari-oblongæ, plano-convexæ: *radiculâ* cylindricâ, cotyledonibus paulò longiori, obtusâ, hilo proximâ. Herbæ (Amer. Austr.) *glandulosæ*, *radice fibrosâ*, *sæpiùs annuâ*. Folia *sparsa*, *integerrima*, *nunc rarò subopposita*. Flores *solitarii*, *pedunculati*, *speciosi*, *albi v. purpurei*.

Obs. A Nicotianâ calycis laciniis foliaceis tantùm differt. Salpiglosis solummodò distinguitur corollâ irregulari subbilabiatâ et calycis non limbo foliaceo. Sectiones Nicotianæ potiùs quàm diversa genera viduntur.

A careful examination of the various species of *Nierembergia* and *Petunia*, which have fallen under my notice, has led me to the conclusion that they constitute but one genus, which is scarcely of itself to be distinguished from *Nicotiana*, except by the foliaceous segments of its calyx. The tube of the corolla is found to be more or less dilated in different species, but this character cannot be regarded as of more than specific importance. The *Nicotiana minima* of Molina is identical with *Nierembergia repens. Salpiglossis* agrees with *Nicotiana* in the structure of its calyx, and is only distinguished from it and *Nierembergia* by its somewhat irregular corolla.

PITAVIA, *Mol.*

GALVEZIA, *Ruiz et Pavon.* non *Juss.*

Syst. Linn. OCTANDRIA MONOGYNIA.

Ord. Nat. RUTACEÆ, *Nobis.*

Calyx 4-fidus: *laciniis* æstivatione imbricatis. *Petala* 4, calycinis laciniis alterna. *Stamina* 8, perigyna, inæqualia: *alterna* petalis opposita, breviora: *filamenta* compressa, glabra: *antheræ* cordatæ, biloculares: *loculis* exteriùs longitudinaliter dehiscentibus, apice vix confluentibus. *Ovarium* 4-loculare, loculorum apicibus parùm productis, disco elevato cylindraceo impositum: *ovulis* solitariis. *Stylus* (e 4 conflatus) 4-sulcatus, basi fissurâ dehiscens. *Stigma* subcapitatum, obsoletè 4-lobum, minutè papillosum. *Bacca* abortu sæpè monococca: *coccis* monospermis.

Arbor (Chilensis) *sempervirens, 6-orgyalis.* Ramuli *tricarinati.* Folia *terna, petiolata, lanceolata, obtusa, crenata, coriacea, glabra, pellucido-punctata, 3-5-uncialia, basi acuta.* Petioli *subtùs obtusè carinati, semipollicares.* Flores *albi, paniculati.* Paniculæ *parvæ, axillares.* Pedunculi *pubescentes.* Calycis laciniæ *subrotundæ, concavæ.* Petala *ovali-oblonga, concava, pellucido-punctata, margine tenuissimè ciliata.*

1. P. punctata, *Mol. Chil.* ed. 2. p. 287.

Galvezia punctata, *Ruiz et Pavon Syst. Veg. Fl. Peruv. et Chil.* 1. p. 97. *Fl. Peruv.* tom. 4. ined. t. 344.

Hab. In Conceptionis Chili provinciâ in stagnatis et ad rivulorum margines. *Ruiz et Pavon* ♄. Floret Octobri et Novembri. Vernaculè Pitao. (V. s. sp. in Herb. Lamb.)

Obs. Genus Calodendro *Thunb.* proximum, similiter folia opposita v. terna, atque habitu alioqui convenit. Folia inter manus contrita fragrantissima.

This genus clearly appertains to the *Rutaceæ*. Its habit is entirely that of *Calodendrum*. The descriptions hitherto published of the genus are very imperfect, and it is hoped the present will supply many of their defects, although we have still to regret the want of perfect seeds to complete our knowledge of its structure. The name applied to the genus by Molina is here adopted, in preference to that of Ruiz and Pavon, as being considerably anterior to the latter, and there being already another *Galvezia* among the *Scrophularinæ*.

AITONIA, *Linn. fil.*

Syst. Linn. OCTANDRIA MONOGYNIA.

Ord. Nat. RUTACEÆ, *Nobis.*

Calyx 4-fidus. *Petala* 4, calycis laciniis alterna. *Stamina* 8, corollâ longiora, declinata: *filamenta* glabra, basi dilatata, connata: *antheræ* incumbentes, lineares, basi apiceque obtusis, biloculares: *loculis* parallelis, distinctis, raphe angustissimâ connexis, longitudinaliter dehiscentibus: *valvulis* cartilagineis, æqualibus. *Ovarium* globosum, 4-loculare, disco parùm elevato marginato insertum: *ovulis* erectis, paucissimis, axi loculorum insertis. *Stylus* filiformis, declinatus, glaber. *Stigma* clavatum, disco parùm depressum. *Capsula* 4-alata, 4-locularis: *loculis* dispermis. *Semina* orbiculata, compressa: *testâ* coriaceâ, fuscâ, extùs tuberculatâ, intùs pelliculâ lævissimâ vestitâ: *albumen* nullum. *Embryo* erectus: *cotyledones* foliaceæ: *radiculâ* his triplò breviori, subconicâ. *Plumula* inconspicua?

Frutex (Capensis) *ramosissimus, 8-pedalis.* Folia *alterna, lineari-lanceolata, integerrima, pellucido-punctata, e ramulis nondùm evolutis plerumque fasciculata.* Stipulæ *nullæ.* Flores *in ramulis nondùm evolutis terminales, solitarii, pedunculati, penduli.* Corolla *campanulata, sordidè purpurea.* Capsula *magna, membranacea, inflata, colorata, reticulato-venosissima.*

Obs. Genus Melianthio calyce et capsulâ proximè affine. Flores rarò 5-fidi, 10-andri.

1. A. Capensis, *Linn. fil. Suppl. Pl.* p. 303.

Hab. Ad Promontorium Bonæ Spei in Karro deserto inter ripas fluminis Gand et Lang-Kloof. *D. Niven.* ♄. (V. v. c. et s. sp. in Herb. Lamb.)

M. de Jussieu has referred this genus to the *Meliaceæ*, and in this opinion he has been followed by most subsequent writers: it appears to me, however, to approach much nearer to the *Rutaceæ* than to any other family; and I am led to believe, after a careful comparison of their several characters, that few will question the propriety of the place that is here assigned to it. The genus has been omitted by M. De Candolle, in the published volumes of his invaluable *Prodromus;* so it is probable that distinguished botanist had not made up his mind with respect to its affinities. Notwithstanding the difference in habit and the structure of their leaves, I am inclined to think that *Aitonia* and *Melianthus* may be included in the same natural family, agreeing, as they apparently do, in the more essential parts of their structure; nor do I think the presence or absence of stipules alone sufficient to separate the *Zygophylleæ* from the *Rutaceæ*, to whom they are otherwise so intimately allied. These appendages in some families are evidently of secondary importance only.

CITRONELLA.

VILLARESIA, *Ruiz et Pavon. Fl. Peruv.* non *Prod.* nec *Syst. eorund.*
CITRI, sp. *Mol.*

Syst. Linn. PENTANDRIA MONOGYNIA.
Ord. Nat. AQUIFOLIACEÆ, *Ad. de Juss.*

Calyx 5-partitus. *Petala* 5, calycis laciniis alterna, disco lineâ elevatâ (quasi filamenta alia iisdem adnata!) bicarinatâ aucta. *Stamina* 5, petalis alterna: *filamenta* planiuscula, glabra, suprà leviter canaliculata, infernè dilatata: *antheræ* reniformes, biloculares: *loculis* turgidis, distinctis, longitudinaliter dehiscentibus. *Pollen* granulosum. *Ovarium* liberum, subbiloculare: *ovulis* subsolitariis, pendulis. *Stylus* brevissimus. *Stigma* obliquè capitatum, minutè papillosum. *Drupa* monosperma. *Albumen* carnosum. *Embryo* minutus. *Radicula* supera, cotyledonibus orbiculatis duplò longior.

Arbor (Chilensis) *procera, sempervirens, comâ frondosissimâ, cortice crassiusculo, rugoso.* Ramuli *pubescentes.* Folia *alterna, petiolata, elliptica, mucronata, valdè coriacea, glabra, viridia, siccitate æruginosa v. flavicantia, suprà lucida, subtùs opaca, pallidiora, margine callosa, integerrima, nunc rariùs dentato-spinulosa, basi rotundata; in* arbore juveni *cordata, margine copiosè dentato-spinosa, undulata.* Flores *parvi, albi, capitati.* Capitula *longiùs pedunculata, axi communi elongato, pubescenti, racemoso-paniculata, terminalia.* Calycis laciniæ *subrotundæ, membranaceæ, concavæ, ciliatæ, æstivatione imbricatæ.* Petala *obovato-oblonga, concava, margine tenuissimè serrulata.* Drupa *parva,* Lauri nobilis *magnitudine.*

1. C. mucronata.

Villaresia mucronata, *Ruiz et Pavon Fl. Peruv. et Chil.* 3. p. 9. t. 231. f. 6. *Ad. de Juss. in Ann. des Scien. Nat.* 25. p. 14. t. 3. f. 2.

Citrus chilensis, *Mol. Chil. ed.* 2. p. 293.

Hab. in Chili sylvis. *Molina, Ruiz et Pavon, Cuming.* ♄. *Indigenis* Guillipatagua s. Guillin; *Hispanis* vernaculè Naranjilla. (V. s. sp. in Herb. Lamb.)

The species on which *Villaresia* was originally founded by the authors of the *Flora Peruviana*, namely *V. emarginata*, evidently belonging to a very different genus, apparently akin to *Celastrus*, from which it appears to be principally distinguished by the bivalvular dehiscence of its capsule, has rendered another generic appellation necessary for their *V. mucronata*; I have, therefore, proposed to call it *Citronella*, being a version of its Spanish name in Chili. The genus I consider to be nearly related to *Cassine*. Ruiz and Pavon state their *Vil-*

laresia emarginata to be a shrub about two yards high, and a native of Pillao in Peru; and the descriptions of the plant in their *Prodromus* and *Systema* shew it to belong to a very different genus from the one subsequently adopted by them in the third volume of their *Flora*. The specimens and drawing of the former were doubtless lost in the shipwreck of the San Pedro de Alcantara. Among a small collection from South America, I have seen specimens of a plant, which may eventually prove to be a congener of *Villaresia emarginata*.

MALESHERBIA.

I have to add the following description of another new species to this interesting genus, for samples of which I am indebted to my enterprising friend Mr Bollaert, who collected them in Southern Peru. The flowers of this new species are pink, and the root is apparently annual. It is readily distinguished from all those hitherto described by its almost pinnate leaves.

7. M. *tenuifolia*, foliis subpinnatis: segmentis linearibus, perianthiis tubulosis, coronâ inciso-lobatâ.

Hab. In Peruviæ provinciâ australiori Tarapacensi ad Huataconda. *D. Bollaert.* ⊙ Floret Februario. *Vulgò* Agi de Zorra, *i. e.* Capsicum Alopecis. (V. s. sp. in Herb. Lamb.)

Herba annua, pube subtilissimâ, præsertim in statu juvenili canescens. *Caules* erecti, ramosi, teretes, palmares, vix pennam corvinam crassitie adæquantes. *Ramuli* axillares, uniflori, subfastigiati. *Folia* alterna, profundè pinnatifida, ferè omninò pinnata, uncialia: *segmentis* lineari-angustissimis, obtusis, integerrimis, canaliculatis. *Pedunculi* brevissimi, apice geniculati. *Perianthium* tubulosum, sesquiunciale, rubicundum, minutè puberulum: *tubus* 10-nervius: *limbo* 10-partito: *segmentis* 5 *exterioribus* elliptico-oblongis, obtusè mucronulatis, 5-nerviis (nervis ramosis, alternis 2 subsimplicibus, vix ultra medium manifestis); 5 *interioribus* petaloideis, ovalibus, obtusis, trinerviis (nervis ramosis), basi parùm angustatis. *Corona* simplex, tenuissimè membranacea, limbo brevior, margine inciso-lobata, erosèque dentata. *Stamina* 5: *filamenta* compressa, glabra: *antheræ* incumbentes, lineari-oblongæ, biloculares: *loculis* parallelis, longitudinaliter dehiscentibus. *Styli* 3, laterales, longissimi, filiformes glabri, arcuato-patentes. *Stigmata* simplicia, obtusa. *Capsula* trigona, pollicaris: *valvis* lanceolatis, acutis, ultra limbum perianthii prominulis. *Semina* plura, ovoidea, gilva, sulcis plurimis notata.

Observations upon the Structure and Development of the Infusoria. By Dr Rudolph Wagner of Erlangen.

The following observations were made in the summer of 1831, and I have to regret that the microscope which was employed cannot be compared with that by means of which Professor Ehrenberg of Berlin was enabled to make his masterly researches into this department of nature. The instrument employed was good, but of the old construction. The object-lenses were manufactured by Utzschneider and Frauenhofer; and, although their defining power was excellent, they did not magnify more than from forty-eight to sixty diameters.

I. *Development of the Hydatina Senta* *.

The *Hydatina Senta* is not unfrequently found in the neighbourhood of Erlangen, in pools and marshes, the surface of which is covered with duck-weed, but it is not common in these situations, being more frequently obtained by keeping water enclosed in covered glasses in the house for some days after the successive production and disappearance of other infusorial forms. This was the case with a natural infusion in which I detected, on the 3d November, a tolerable number of the *Hydatina.* These were of all sizes; some were visible to the naked eye, but still more distinctly with a common pocket lens. Some of the animals swam about in the water, but the greater number attached themselves by their posterior extremity to the walls of the vessel and vibrated freely the anterior part of their bodies, in all directions, in the fluid. On the sides of the glass, and still more abundantly towards the surface of the water, was a brownish pulverulent mass, interspersed with small globular greenish grains,—the dead bodies of the *Euglena viridis*, Ehrenberg. When a portion of this mass was placed under the field of the microscope, I observed a number of darker coloured and more distinctly defined oval or rounded bodies, which I immediately recognised to be ova, and there was visible besides a number of empty sacks, split up the middle, which had evidently been the envelopes of former ova. On the following day, the 4th November, the number of the animalcules had increased, as well as that of the empty sacks adhering to the sides of the glass. A number of new eggs had also been laid, which were chiefly collected towards the surface of the water. A glass with a magnifying power of three, distinctly shewed, in the grown animals, a nearly ripe ovum; others were destitute of the round dark point beside the intestinal canal; these had probably a short time before evacuated their ova. Besides the *Hydatina*, there were many still smaller infusoria, which swam about with much vigour, and appeared, under the microscope, like the *Vorticella larva*, Müller; their eggs seemed to be much smaller and more spherical, interspersed among the larger ova. On the 5th November, still more eggs had been laid, and new animals excluded; the numbers of the *Vorticella larva* had much diminished. On the 6th, many more animals seemed to have been excluded, but no new eggs had been laid. The *Vort. larva* had almost entirely disappeared from the infusion. On the 8th, the number of animalcules remaining was comparatively small. It continued nearly stationary to the 11th, on which day no new eggs were found on the sides of the vessel, although the dark spot (ovarium) beside the intestinal canal, was very distinct. On the 20th November very few individuals remained, and no new eggs had been laid.

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* Figures illustrative of the external form and internal structure of the *Hydatina senta* are given in Pl. IV. of the preceding volume of this Journal.

With regard to the size of the animal, my observations are not quite in accordance with those of Ehrenberg. In his table, he states it at from $\frac{1}{6}'''$ to $\frac{1}{3}'''$. I usually found them larger; the largest individuals were very commonly $\frac{1}{4}'''$, some almost $\frac{1}{3}'''$, more exactly $\frac{7}{20}'''$. The ova could be detected with the naked eye, when the glass was held up to the light, like small dark points adhering to its sides; and my eye is capable of distinguishing objects of the size of $\frac{1}{30}'''$, when close to the eye, and in active motion like the infusoria. With a lens magnifying three times, the eggs appeared like small disks, and the empty sacks could be distinctly perceived adhering to the sides of the glass. Under the microscope, their sizes varied from $\frac{1}{10}'''$ to $\frac{1}{20}'''$, without, however, their magnitude appearing to depend upon their internal development; for I found ripe embryos both in the small and large ova. Their form was generally oval, approaching more or less to a circle. Their colour was often dark, yellowish-brown, and opaque; some were partially of a lighter colour, particularly at the margins; others throughout of a lighter colour. No trace of motion was observable in the dark ones, while in the latter the embryo often made vigorous movements, and the ciliæ were seen distinctly vibrating in the interior of the egg. The former were therefore evidently the least developed ova. We may therefore perhaps form the following scale of their development:

1. The dark opaque ova I reckon the most unripe; they vary in size, and are more or less elongated. When pressed between plates of mica, the granular homogeneous-looking substance in the interior is discharged (the yolk), and the empty transparent envelope remains (the chorion). I could never observe any thing more than this granular substance, and its containing membrane. I could never detect a second coat, the amnion of Ehrenberg. The chorion had a remarkable appearance, being beset with short, fine, dense hairs, resembling the closest fur. This hairy covering is very apt to escape notice. I did not observe it at first; but, after having once seen it, I found it present in every case. This villous covering, which reminds us so closely of the chorion of the higher animals, is most distinct in unbroken opaque ova. It is, however, visible, though much less distinctly, when their contents are discharged. This structure is the more interesting, as Meyer informs us (Okens Isis, 1828, p. 1225), that the envelope of the egg (chorion) of the *Alcyonella* is beset with ciliæ. Grant found it on the ova of the sponge, and Carus views it as the nucleus of the respiratory vessels or gills of the chorion.

2. An egg has sometimes an opaque nucleus and border, when the rest of the granular mass is transparent. This also was beset with hairs. I am not sure whether this is the second stage of the development or a dead ovum.

3. The ova of this stage are much more transparent, filled with a brownish-yellow, granular substance (yolk); some spots are darker, and resemble turbid flocci in the midst of the ovum. Some organization is discernible in the interior, but there is no distinct separation of organs. Some resemblance seems to exist to the embryo of the common leech, where the first change perceptible in the globular ovum is the formation of cells and vesicles.

4. The nearly ripe eggs are the most transparent, particularly on their margin. The embryo is often perfectly formed, quite transparent, and the ciliæ are in active motion in the interior of the egg. The head of the animal, on the rotatory organ, is generally turned towards one or other of the ends of the oval eggs. Frequently the embryo turns quite round in the egg, by means of a sudden motion bringing its rotatory organ to the opposite extremity. I gently pressed several of the ova between plates of mica, and was fortunate enough to split the envelope, so that the embryo started out; it was commonly very much contracted, the tail drawn in, so that I imagine the embryo does not lie curved on itself, but contracted; but I may be deceived in this as in other cases. I thought that I observed a turned posture. The teeth and the mouth seemed to be the first formed; for, on one occasion, I pressed out of the egg a fetus which had not yet begun to vibrate its ciliæ, but in whom these organs, as well as the bifurcated tail, were quite distinct. In those who had begun to move their rotatory organ in the egg, I recognised, in addition, the perfectly formed colourless intestine and the ovary. Twice I saw the exclusion of the embryo. The shell split transversely, and the animal immediately protruded its rotatory organ, and in about a minute slowly extricated itself from the egg, extended itself at full length, and began to swim slowly forwards. All its parts were perfectly formed; it was quite transparent, and was only at first a little curved. But the intestine was constantly quite colourless. The largest was about $\frac{1}{9}'''$ long. The remnant of the envelope of the egg was transparent and fissured in different directions.

The development of the ova in the ovarium was another subject of my observations. Even in animals just excluded from the egg, I could observe the individual ova in their interior. By slight pressure of the animalcule between plates of mica, the ovarium, as well as the intestine, was rendered very distinct, and by attentive inspection, there could be detected seven very small oval ovules, which had a black spot in their middle, which I reckon their yolk. Similar ovules were perceptible in an animalcule about $\frac{1}{7}'''$ long, which having already eat, had its intestine of a greenish colour. In a grown animalcule of $\frac{1}{3}'''$, in which the intestine was quite filled with dead *Euglenæ*, I saw the ovary to contain nine eggs, of different degrees of ripeness as well as size. The smaller had a distinctly granular interior, and a dark point in the centre, the larger were quite opake. The animal evacuated the contents of its intestine by an anus, and also excluded the ova by means of the cloaca and the same opening. During this operation I observed the distinct contraction of the vesicular organ seated below the organs of generation, and which Ehrenberg views as the *musculus ejaculator seminis*. The small ova escaped most easily, the larger were a somewhat longer time of making their exit, and usually broke during the passage, and evacuated their granular contents. If the uterus was distinctly two-horned, as in Ehrenberg's figure, each horn contained four eggs, the lowest one was the largest ($\frac{1}{30}'''$), most transparent at the edges, and with the dark granular mass still in the interior. The three uppermost eggs were commonly oval, and had also a central darker nucleus. The ovarium thus contained simultaneously ova in very different stages of development.

Much similarity will be found to exist between these fragments of a history of the development of a *Hydatina* and the tables which Carus has given of the *Lacinularia fluviatilis*. Here the egg is formed of a chorion and an albuminous mass (yolk), which forms the embryo. The rotatory organ here also moves in the egg, from which Carus concludes this oscillation to be the respiratory act, which thus takes place in the fetus before any nutrition can be conceived to take place by the mouth. I am also disposed to adopt the same view of the rotatory organ being the apparatus for respiration, but its complete establishment would require some connection to be traced between the respiratory and the circulatory systems, and a more complete demonstration of the existence of the latter than we as yet possess. Something similar seems to occur in the embryos of the leech, which, according to Weber, move about and swallow albuminous matter.

II. *Eggs of the Vorticella larva*, Müll.

I have already mentioned that there existed cotemporaneously with the *Hydatina senta* in the water, a smaller animal, which exactly agreed with the *Vorticella larva*, as figured in the En-

cyclopedie Methodique, Pl. 21, Fig. 9. The animal, including its tail, was $\frac{1}{12}'''$ long, and exhibited in its interior a distinct intestine and ovarium. In the midst of the eggs of the hydatina there were now a number of smaller, rounder, more transparent bodies, but also filled with the granular mass Precisely measured, they were about $\frac{1}{40}'''$, and in some I could perceive the motions of the embryo. I never saw the act of exclusion, but have no hesitation in setting down these bodies as the eggs of the *Vort. larva.*

III. *Intestine, and Experiments on Nutrition with Carmine in some other Infusoria.*

Feeding by means of a solution of carmine, completely succeeded with me in the larger Infusoria. In the smaller, such as the Monads, *Cyclidium glaucoma*, &c. &c. I could not repeat Ehrenberg's experiments, as I had not the means of employing sufficient magnifying powers, although the animals themselves, even the little *Vibrio aceti* were quite distinct under my microscope. In the *Paramæcium*, *Trachelius*, *Kerona*, *Euplæa* and *Vorticella*, however, with a power of 50–60, their filling with red colouring matter was quite apparent. Ehrenberg recommends for this purpose the pedunculated Vorticellæ; but I could never succeed with them so well as with some of the others, probably because I never found the *Vort. convallaria* so large as Ehrenberg mentions in his table ($\frac{1}{24}'''$). I seldom found them to exceed $\frac{1}{40}'''$, but here without feeding, I could distinctly see the stomachs like round vesicles, and when filled with carmine I counted fourteen. In the *Euplæa* I counted twenty red points filled with carmine. But the experiment succeeded in the most beautiful and perfect manner with a *Paramæcium*, probably the *chrysalis*, which, on one occasion, I found of unusual size, from $\frac{1}{8}'''$ to $\frac{1}{10}'''$. The red-coloured sacks were quite apparent with a power of 24, and very distinct with one of 48. The animalcule itself can easily be seen with the naked eye. When the intestine is once filled, it remains for several days equally distinct. I therefore recommend this infusory animal to any one who is desirous of examining their structure, and of exhibiting it distinctly to others.

Just as little as Ehrenberg could I ever succeed in inducing the green-coloured infusoria to swallow colouring matter. In the *Euglena viridis*, Ehr. (*Cercaria viridis*, Müll), I recognised distinctly the red eyes, but could never detect any other red point, however long it was left in the solution of carmine. The remarkable *Vorticella versatilis* or *Ophrydium versat.* Ehr. could never either be induced to take the colouring matter. I found this species in great numbers towards the end of October, forming gelatinous balls of an inch and a half in diameter; they were best adapted for experiment, when their size was about $\frac{1}{10}'''$. This wonderful animal is of a beautiful green colour, paler, and more transparent near the edges: their interior consists of a granular matter, or of small round vesicles (?)

IV. *On the Change of Form, and the Successions of Infusoria in Infusions.*

It appears very probable, from the observations of Ehrenberg, that the genus *Monas*, and several of the allied genera, are not distinct animal forms, but merely the young state of the *Kolpoda*, *Paramæcia*, &c., which are unable to reach maturity without a division like the *Rhizomorpha* and *Byssus* among the Fungi. This idea has been subsequently adopted by Dr Eschweiler. I have myself no continued observations on the primitive generation of the Infusoria, but for several summers the appearance mentioned by Eschweiler * has appeared to me very remarkable. I have seen monads and other small infusoria appear in infusions which disappeared in a few days, and gave place to other genera. Thus the water above mentioned as containing the *Hydatina*, on the second day after being taken from the pool, formed a green crust on its surface, particularly near the margin of the glass in which it was kept. When I examined this green mass with the microscope on the 23d October, it swarmed with the *Euglena viridis* and *Enchelys pulvisculus*, which swam about with much activity. Some of the first species were seen in the act of expiring, and others quite dead. During this curious phenomenon the elongated form of the animals was changed into a globular. They died in whole masses, particularly near the margin of the vessel; the green crust then forming the so-called Priestley's matter, although it may arise as well from other green Infusoria. Another animalcule seen in considerable quantity was the *Volvox globator* of different sizes. On the 27th October the green mass of dead infusoria was still present, and formed a pulverulent layer on the side of the glass turned towards the light. On the surface there was a pellicle which exhibited numbers of the *Kerone pustulata* of $\frac{1}{3}'''$ in size. Some had a transverse fissure, as figured by Ehrenberg; and there were a few examples of *Vorticella larva*, Müller. On the 3d November, not only the *Euglena*, but also the *Kerone pustulata*, had entirely disappeared. A new set of organisms had sprung up; the dead *Euglenæ* still retained their green colour and rounded form, and lay on the sides of the glass; many of the green masses, however, had become brown and yellow, and had aggregated together. Here and there I saw a *Paramæcium*, and more commonly the *Vorticella larva*, the interior of which was filled with dead *Euglenæ*. There were also a few of the *Hydatina senta*. The *Vorticella larva* gradually disappeared as the *Hydatina* increased; and the latter at last vanished, so that, on the 10th November, the water was quite destitute of infusoria. Similar successions, though not in the particular order of this example, I have seen on different occasions. At other times I have observed several infusoria cotemporaneously existing together, as the *Monas*, *Vorticella*, *Trachelius*, *Kolpoda*, *Kerone*, *Paramæcium*, all of which disappeared at once. Certainly these transient swarms of living beings within the compass of a single glass, is an object of very great interest. We are quite ignorant of its cause. At first sight it might seem to favour the idea of the *generatio equivoca*, but my belief in this hypothesis has been pretty well shaken by Ehrenberg's numerous observations, as well as by the few which I have been able to institute in the course of a single summer. Nowhere in nature do we see any animal arrive at once at its perfect state; there is always a sort of dormant state previous to their properly independent existence. The invisible animating power of the Creator every where in animated nature attaches itself to the state of the egg, and at this state of existence more especially, does he seem to breathe into living beings the breath of life. The dormant state of the ovum seems to be the transition from the empire of mind to that of matter. The old maxim "Omne vivum ex ovo," is for me, as well as Ehrenberg, equally binding upon the infusorial tribes. I certainly think it very possible that there may be a *generatio equivoca* of eggs, but not of perfect animals; but our data are as yet quite insufficient for the demonstration of the question.

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* Isis. 1831, Heft. iv. p. 403.

V. *On the supposed Connection between the Vegetable and Animal Kingdoms.*

It is very commonly an admitted fact, that vegetables and animals touch one another in their lower degrees—that they even pass into each other,—that not only in one genus do we find both animals and plants, therefore species from the two kingdoms of nature which can be separated by no generic distinction, but that even a plant often produces an animal which, during its growth, again changes into a plant. I must admit that these notions were once very attractive to me; that the globules of the blood were simple monads, which were formed from the living animal matter, like the infusoria from dead animal matter; that the *Ectosperma* produced monads which disappeared after the short life of an hour, and grew up to Confervæ. I not merely believed this myself, but have publicly taught it as an acknowledged fact, till I took the microscope in my hand and experimented for myself. I have now perhaps fallen into the other extreme; for, certainly, I have never succeeded in observing the separation of the globules of the blood from the parenchyma, although I do not pretend to deny its possibility; nor have I at any time seen how animals change into plants; and, notwithstanding Spallanzani's often repeated experiments, doubt very much of the resurrection of *Furcularia* after having been dried up for more than a year. When Treviranus, Bory St Vincent, and others, have seen globules issue from the tubes of the Confervæ and swim freely about like infusory animalcules; when Edwards has seen Confervæ again spring from these globules after settling; when Unger and others have seen active globules, which he calls Monads, issue from the *Ectosperma clavata*; I do not doubt the fact of globules having issued from these cryptogamic plants, which sprung about in different directions: these motions seem voluntary, therefore they are animals; they are very small and round, therefore they are monads. Here, therefore, a moving globule and a monad are considered as identically the same. In order, however, to establish this, they should have been seen to take in colouring matter, and to contain internal cavities, which Ehrenberg has done with regard to all the true monads. Ingenhous, Treviranus, Hornschuch and Nees Von Esenbeck, if I mistake not, have said that the Priestley's matter consists of dead infusoria, and that from this congealed vegetable mass, as it is called, Confervæ, Ulvæ, Tremellæ, nay even the *Hypnum riparia*, have all been formed. Here I do not doubt the fact; times without number have I seen the Priestley's matter take its origin from green infusoria, such as the *Euglena viridis*, and the green pellicle in pools and infusions very generally consists of the dead bodies of infusoria, but never have I seen plants formed from this green matter; it continued long fresh, and became afterwards yellow, brown, run together, and fell to the bottom. When Confervæ grew in the infusion, or in the midst of these masses, there was never a transition between the dead infusoria and the living plants. Whether these plants arose from the spontaneous development of globules, or from the presence of infinitely small seeds in the water when drawn, I will not pretend to determine. Still less can I believe in the observations of Wiegmann, that the *Entomostraci*, *Cypris* and *Cyclops* are generated in putrid water from Priestley's matter, and after their death are changed into confervæ. He even asserts that he has seen confervæ springing out of the feet and antennæ. It is very possible that these animals have arisen *on* confervæ, but not *from* them.

VI. *Structure of the Cercariæ.*

The exact observations and inimitable drawings which have been given of the Cercariæ by Nitsch, must be known to every one who has occupied himself with researches into the infusoria. Subsequently Baer has made some highly important observations on their formation, after Bojanus had called our attention to the subject. During last summer I had an opportunity of observing the living Cercariæ in some water which had been drawn from a pool for the purpose of examining the infusoria, and which had been at rest in a glass. It contained also many planorbes, and as I think the *Pl. carinatus.* On examining the water in a few days, I observed with the naked eye some very active animalcules, which I immediately recognised as cercariæ by their very rapid motions. Their numbers increased on the following days. My observations were afterwards interrupted. On closer examination the animal appeared to be the *Cercaria Lemna*, Muller (Encyclop. Method. Pl. 8. fig. 8. 12.), a species which, as Nitsch supposes, is probably identical with the *Cercaria major* (Beytr. zur Infusorienkunde, Tab. ii. fig. 1–8.), for it resembles it exactly ; only I could detect none of the fine hairs on the tail, which, however, only appeared to Nitsch with very high powers, and with the field of vision half illuminated. I employed a power of 48. For a long time I was quite delighted with the singular movements of these Cercariæ, but I turned my attention principally to their internal structure, which is not sufficiently known.

The cercaria which I observed was about $\frac{1}{2}'''$ in length. The tail and the body were nearly of the same thickness ; the latter was longitudinally striated. The substance of the animal was homogeneous, transparent, and of a milk-white colour. At the anterior extremity was the round extensile opening of the mouth, from which it appears to be surrounded with a wreath which was notched. This mouth is succeeded by a smaller very distinct œsophagus, between which and the mouth the intestinal canal seemed to be somewhat narrowed. I saw no trace of the forked organ noticed by both Nitsch and Baer ; nor was the course of the intestinal canal here marked by any dark spot, but it appeared to me merely to resemble a broad band extending from the mouth to the anus. The ovaria were very conspicuous, placed on the two sides of the body. When the animal was moderately extended, they made several convolutions in the form of an S, from the two sides of the œsophagus to the cloaca. The convolutions were less distinct when the animal was much extended. The hinder extremity was very short and broad, where the ovaria formed two irregular masses. Their texture appeared granular, and had a beautiful appearance, their dark colour being contrasted with the transparency of the rest of the body. Their exit into the cloaca I could not distinguish. Their origin towards the anterior part of the body was very distinct by two dark spots. Nitsch took these spots for eyes, which they certainly are not, but the origin (if not the exit, against which, however, we have all analogy) of the ovaria, which are here destitute of the granular tissue. These spots sometimes assumed a spiral, sometimes a forked or double forked, form. With regard to the organs of motion, the mouth is large, more or less extensile, and surrounded with a notched or plain margin or ring; it is bell-shaped, and is attached to the body by a narrow pedicel. The tail has lateral indentations and longitudinal striæ or fibres towards its middle. I am inclined to suppose a union of transverse and longitudinal fibres, true muscular tissue, for I am convinced that these animals possess muscles quite analogous to those of the higher animals, and Ehrenberg has demonstrated it with regard to the hydatina. How the tail is fixed to the body, I have not been able precisely to determine, but it is probable that a sort of prolongation is fixed into a notch in the hinder part of the trunk.

These few observations on the infusoria cannot be compared with those of Nitsch, Baer, and Ehrenberg; they are only a fragment towards the completion of the history of this vast kingdom of nature. In conclusion, I must add a word to explain why I have taken no notice of two recent observers of infusoria—Bory St Vincent and Muncke; but so much do I value the labours of O. F. Muller and Schrank, that I owe it to truth to assert that I consider the communications of the former as quite lost for science. When Muncke says that he has devoted from three to four hours every day for three weeks to the observations which were laid before the meeting of Naturalists at Hamburg, we can only lament that a man of acknowledged reputation as a natural philosopher has spent so much time uselessly upon matters with which he was quite unacquainted. It is worse with Bory; for it is quite incomprehensible how any an can be considered as a naturalist of eminence, who disys in his writings an inaccuracy, superficiality, and ignorance, t must be frightful to every one who examines closely his ks, and as revolting a [illegible] frivolity of which his Histoire relle de l'Homme is [illegible]; an example.

An Exposition of some of the Laws and Phenomena of MAGNETIC INDUCTION, *with original Illustrative Experiments.*

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THE magnetic principle, like the electric, to which it is nearly allied, is not a mere attribute of a particular class of bodies, but a principle or influence pervading, doubtless, the whole of the terrestrial creation. For it is not likely that such an influence has been ordained by Infinite Wisdom for the comparatively minor purposes to which men are able to apply it; but rather that it is an essential constituent in the economy of the globe,—and not of this globe only, but of the entire system, perhaps, of created nature. Wherever the exploring traveller has urged his way, there its influences have been marked; wherever the adventurous mariner has traversed the ocean, there its agency has availed him; wherever the laborious miner has penetrated the earth, there its energy has been found undiminished; and wherever the daring aeronaut has ascended into the atmosphere, thence its power has been extended.

Though a universal agent, however, and a part of the constitution of our globe, it is chiefly in ferruginous bodies, and in bodies in a peculiar electric condition, where its phenomena become sensible, and its influences capable of being controlled. In ferruginous bodies, its strongest and most permanent energies are exhibited.

In iron, the magnetic principle has evidently permanent residence,—capable, indeed, of exerting external influences, but not capable of being abstracted or increased. Each portion and description of iron has its own constant and unalterable quantity, abiding apparently in its individual particles,—the two qualities possessing northern and southern polarity existing in every particle. The usual condition of the magnetisms is generally neutral, or nearly so, so that but a slight and imperfect energy is naturally evinced. Yet the latent energies, especially in the softest kinds of iron and steel, are readily developed by electric influence, or by the touch, or even the mere juxta-position of an active magnet. For though the natural quantity of inherent magnetism cannot be altered, yet an active and constant influence may be, and is, externally exerted, without in the smallest degree diminishing the original power of the actuating magnet. That this is the case, a well known fact sufficiently proves,—that a magnet may elicit the magnetic condition in ten thousand bars of steel, and yet retain its original strength perfectly unimpaired. And that the magnetisms reside inalienably in their own particles of the metal, is equally evident from another familiar fact,—that if a bar in a magnetic condition be broken in the centre, so as to separate apparently the northern and southern polarities, each portion, instead of comprising one quality only, will be found to be a distinct magnet, exhibiting, like the original mass, the two different polarities at the extremities.

These general principles being premised, we are prepared for the consideration of the particular objects of this Essay.

CHAP. I.—EXPOSITION OF SOME OF THE LAWS AND PHENOMENA OF MAGNETIC INDUCTION.

INDUCTION is that well known property of magnets, of producing in contiguous ferruginous substances the magnetic condition. It is not, however, strictly speaking, the communication of any thing previously foreign to the ferruginous bodies, as nothing (as we have shewn) is abstracted from the inducing magnet, and nothing in reality infused into the iron thus magnetized. Induced magnetism, therefore, may be defined,—*the development of the latent magnetism in iron or steel, by the juxtaposition of any substance in a magnetic condition.* For this property is elicited not only by actual magnets, but by all electro-magnetic or thermo-magnetic arrangements. And the inductive effect is produced upon all substances capable of a magnetic condition, according to their respective susceptibility. The degrees of capacity of different ferruginous bodies for magnetism by induction may be ranged, beginning with the least susceptible, in the following order; iron-ores, hard cast iron, hard cast steel, hard blistered steel, soft steel, common malleable iron, best Swedish iron,—the most susceptible of all being the softest and most ductile iron. Hence it is probable, that the measure of the relative capabilities of iron for induced magnetism, may be employed as a satisfactory test of quality in its several kinds.

At small distances from a powerful magnet the inductive energy is very striking, and productive of a number of well known and interesting phenomena. Let a piece of soft iron, for instance, such as a common key, be placed near the extremity of a bar-magnet, either in continuation of the same line or inclined to each other at any angle, provided either of the *extremities* of the key and magnet are the parts nearest to each other, and the key so placed will instantly acquire such a magnetic condition as to be able to support another smaller key or other portion of iron at either of its extremities. And though the magnet be placed beneath the table, or under a slab of marble or any other solid substance, the inductive influence will be precisely the same, so as the distance and position of the masses are similar.

But these phenomena, which have usually been observed only in circumstances of juxtaposition, may be satisfactorily exhibited at considerable distances, as shewn by the following experiment.

A pair of very fine bar-magnets, 3 feet in length, 2¼ inches in breadth, and ¼th of an inch in thickness, being placed over each other, a quarter of an inch asunder, and with similar poles adjacent, were employed, with a view to their inductive effects, at different distances, on the nearest end of a soft bar of square iron, 13¾ inches long, and 1 inch in thickness. In order that the inductive influence might be separated from any magnetism of position derived from the earth, the iron bar was placed in the east and west magnetic line, and in a horizontal position; and that the more delicate experiments might not be affected by the magnetism acquired under the more powerful influences, the most distant and weakest inductions were first tried, and each experiment verified by taking the mean power of each end of the bar alternately presented to the magnets. The iron bar thus situated (Plate II. Fig. 1.) sustained by the extremity nearest to the magnets (when all the points of contact were polished) the substances and weights, at the distances respectively annexed to each, as given in the first three columns of the following table:—

Articles suspended by the Iron Bar.	Weight suspended	Distance of the Bar and Magnets on the same level.	Distance when the Bar was raised.
	Grains.	Inches.	Inches.
A piece of wire, ...	0.9	25.5	25.5
Polished nail, ...	20	21.0	21.0
Ditto, ...	37	17.5	17.5
Ditto, ...	112	9.5	10.8
Ditto, ...	194	5.2	7.0
Ditto, ...	389	4.4	6.8
Ditto, ...	482	4.2	6.5
A key, ...	1200	2.5	5.3

But these results, if we consider the nature of the polarities induced in the bar and the suspended substance in their relation to each other, will be seen to be less than what might be obtained by simple induction at the respective distances. For, let the acting end of the magnets be the north pole, then the magnetism induced in the nearest end of the iron bar will be the contrary, or southern polarity; but the tendency of the magnets on the end of the nail, or other suspended substance, in contact with the bar, will be to produce also southern polarity,—because of this end of the nail, under the circumstances, being nearer to the magnets than the lower end; consequently, the induced magnetisms of the parts of contact of the bar and nail, being, as far as derived from the magnet, of the same kind, must have a tendency to diminish the action of the bar on the nail.

In order to compensate this defect in the results, another series of experiments was made with the same apparatus, in which the iron bar, whilst kept in the same vertical plane, was raised on each trial so much above the level of the magnets,—namely, one half the length of each suspended nail,—that the influence of the magnets upon the suspended body might be similar at both its extremities, and consequently neutral. Under this arrangement (Plate II. Fig. 2.) the inductive power, as had been anticipated, was much greater, so that the different substances were now suspended according to the distances in the last column of the table.

Whilst these striking results were obtained by the action of the end of the bar nearest to the magnets, the influence at the distant end was but feeble—sustaining only nails of 20 grains, and 37 grains, at the two last distances of the third column.

In experiments merely illustrative of the *power* of induction, the arrangement of the last column of the table may be still farther improved by raising the bar so high that the lower extremity of the suspended substance may be on the same horizontal level as the magnets. In this position the lower extremity of the nail being nearest to the magnets, becomes a south pole, with a north pole, by consequence, at the upper end, which, being attractable by the southern polarity induced in the bar, augments its suspensive power. And such was the increase obtained by experiment, that the nail of 482 grains was now suspended (by either end of the bar, when presented in succession to the magnet), at the distance of nine inches, and the key at the distance of 7.2 inches, the interval between the bar and the magnets.

Subsequent experiments were made with a flat iron bar, a quarter of an inch in thickness, but similar in all its other dimensions and quality to the bar previously employed. The distances of suspension obtained by this bar, indicated a smaller quantity of inductive influence, corresponding pretty nearly with its proportion of surface.

These, however, are but proximate results: the inductive effect as measured by the deviations produced on the needle of a compass was therefore resorted to for a more satisfactory ratio.

The apparatus employed in this investigation consisted of four similar bar-magnets, each 13¾ inches long, 1 inch broad, and ¼th of an inch thick; the square bar of soft iron used in the foregoing experiments, being of the same length and breadth as the magnets; and a delicate compass of four inches diameter. The magnets were placed parallel to each other on a board, half an inch asunder, in order both that their combined influence might be employed for affording more decisive results, and that they might be presented at different distances from the bar in the same relative position and equality of magnetic power. The iron bar was placed in a horizontal position, in the first instance betwixt the magnets and the compass, and in an east and west magnetic direction, where, being in the plane of the magnetic equator, it was free from any disturbing influence from terrestrial magnetism. The magnets were then placed at various successive distances in the line of the bar, according to the arrangement represented in Pl. II. Fig. 3. The distance of the bar from the compass, which throughout the series of experiments was always precisely the same, was $5\frac{1}{4}$ inches, and the distance of the magnets from the bar was first 12 inches, and then at equal successive intervals, as far as four feet. The object of fixing upon intervals of 12 inches was, because that distance was very nearly the *focal length* of the magnets, or the length betwixt the two foci or centres of attraction in either half of the magnets, to which the whole of the varying magnetic influences, in such segments, might be referred. The employment of intervals, therefore, of focal lengths of the magnet and iron bar, was calculated to simplify the investigation, and to afford more satisfactory results. Though I had a motive for fixing upon the position given to the iron bar, the distance of its focal point from the centre of the compass being about one-half of its focal length; yet, whilst its position was, on every trial, always the same, the particular distance was of little importance.

In this position of the bar, the first situation of the magnets was $(5\frac{1}{4} + 13\frac{3}{4} + 12) = 31$ inches from the centre of the compass; the second 43; the third 55; and the fourth 67 inches. Placing now the magnets, in the general line, with their south poles directed towards the east, at the shortest interval, the bar not being yet placed intermediately, the power of the magnets alone, as determined by their action on the needle of the compass, was first observed, and found to be 27° 42′. The iron-bar, the magnetism of which had been carefully neutralized, was then interposed in its assigned position (according to Fig. 3.), each end of the bar being alternately presented towards the compass, when the mean deviation, by a double set of careful observations, proved to be 38° $3\frac{1}{4}$′. The unassisted action of the magnets was then verified by another trial after the bar was removed, and found to be unaltered; consequently the increase of deviation of $(38° 3\frac{1}{4}' - 27° 42') = 10° 21\frac{1}{4}'$, indicated the proportion of action due to the magnetism induced, under the particular circumstances, into the bar of iron; or rather, (being directly induced into the end of the bar nearest to the magnets), transmitted consequentially to the opposite extremity. For although there is a direct inductive action on the remote extremity, it is the transmitted influence, it is to be observed, which predominates.

In a similar manner, the relative effects of induction were afterwards determined, at the several intervals of two, three, and four feet between the magnets and the bar. The results were as follow :—

No. of Lengths.	Dist. of Magnets from the Iron-bar.	Dist. of Magnets from the Compass.		MEAN DEVIATIONS.				DIFFERENCES	
				By the Magnets alone.	Tangent.	With the Iron-bar interposed.	Tangent.	Of the Deviations.	Of the Tangents.
	Feet.	Ft.	In.						
1	1	2	7	27° 42′	52501	38° 3¼′	78281	10° 21¼′	25780
2	2	3	7	12 45	22628	16 16¼	29187	3 31¼	6559
3	3	4	7	6 41½	11733	8 22	14707	1 40½	2974
4	4	5	7	3 57	6905	4 58⅔	87100	1 1⅔	1805

Now, since a magnetic force or forces of variable intensity, acting upon a compass needle in a direction at right angles to terrestrial magnetism (as in the foregoing experiment), will produce deviations the *tangents* of which will be proportional to the actual intensities; the tangents of the angles of deviation, as above determined, will afford a measure of the relative forces acting upon the needle in the several positions of the magnets and iron-bar; whilst the differences of the tangents of deviations will indicate the quantity and proportion of magnetism induced and *developed in the end nearest the compass*, at the several distances of one, two, three, and four feet. Were the magnetism induced upon one end of a soft iron-bar exhibited, as if by a perfect conductor, to the other end, then would these differences exhibit the actual effects of induction; and the law of induction, as to distance, might be experimentally determined. But this, as will hereafter be shewn, appears not to be the case. Therefore the differences indicative of the actual induction at the remote end of the bar are probably not proportional to the relative forces known to be acting upon the end nearest to the magnets. Still, however, the differences of the tangents in the preceding table afford a ratio not very incongruous with the known law of magnetic influence as to distance. Calling the nearest difference (25780), indicative of the quantity of induced magnetism 100, then the other differences afford a ratio of 25.2, 11.5, and 7.0, which, expressed fractionally, are very nearly 1, $\frac{1}{4}$th, $\frac{1}{9}$th, and $\frac{1}{14}$th, exhibiting the proportion of induced magnetism at the several distances of 1, 2, 3, and 4 lengths.

Whilst the foregoing experiment affords an indication of the relative force of magnetic induction at different distances, approximating the known law of direct attraction and repulsion, in which the forces are inversely proportional to the squares of the distance; it likewise shows the facility with which the magnetic energy induced at one end of a bar of soft iron is conveyed to the other. There was nothing in the experiment, however, which could afford any data for ascertaining whether the transmission of the magnetic energy from end to end of the bar was perfect; or, if not, what proportion of resistance there might be in a bar of soft iron to the free communication of induced magnetism to the remote end of the bar.

With the view of obtaining information on this point, I proceeded to vary the experiment, by ascertaining the deviations produced by *the same end* of the iron-bar as that on which the influence of the magnets was immediately acting; and this was accomplished by the following arrangement.

With the four magnets placed on a tray, exactly as in the former series of experiments, their inductive influence on the bar of soft iron was now examined, when arranged after the manner represented in Fig. 4.

The compass, in this case, being betwixt the magnets and the iron, was placed, as in the former experiment, at the distance of 5¼ inches from the bar; and the intervals were now measured across the compass, and noted at 1, 2, 3, and 4 feet, as before. Under this arrangement, the south poles of the magnets being presented towards the compass, caused the deviation of its north pole, as before, to be towards the west; and this deviation, on the application of the iron-bar, was augmented, because the nearer end of the bar becoming a north pole by induction, assisted by its repulsive action, the westerly deviation of the north end of the needle.

Successive trials were then made on the unaided action of the magnets on the compass at the respective distances, as well as of the increase produced by the mean action of the different ends of the iron-bar. The results are here exhibited.

No. of Lengths.	Dist. of the Magnets from			Influence of the Magnets alone.		Influence of the Magnets augmented by the Iron Bar.		Difference of Deviation	Difference of Tangents
	Iron Bar.	Compass.		Mean Deviation	Tangent.	Mean Deviation.	Tangent.	Col. 4. & 6.	Col. 5. & 7.
	F.	F.	I.						
1	1	0	6¾	86°.40′	1716934	86°.48′	1788631	0°.8′	71697
2	2	1	6¾	60.41	178077	63.18½	198900	2.37½	20823
3	3	2	6¾	28.1	53208	30.42¾	59405	2.41¾	6197
4	4	3	6¾	12.45	22628	14.31¼	25900	1.46¼	3272

Now, the comparative forces, as represented by the difference of tangents, of the magnetism induced into the iron-bar, under the two conditions of *direct induction* on the nearest end, and of *developed induction* on the end most remote from the magnets, were as follows:—

Effects of Induction.	1st Length.	2d Length.	3d Length.	4th Length.
Direct on the nearest end,.....	71697	20823	6197	3272
Ditto, another series *,.........	110104	21840	5832	...
Developed on the farther end,	25780	6559	2974	1805

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* The great difference of the results of the first length betwixt the two series of experiments on the direct induction on the nearest end of the bar, requires explanation. It may arise from two causes, the difficulty of placing the magnets when there were four in number, to act equally and tangentially as one magnet, when the distance between the tray of magnets and the compass was so small, and the importance of the smallest error of observation when the tangents of deviation are so great. The first cause, however, being considered as the principal one, the first experiment of the second series was made in a different manner, by arranging the magnets in a vertical parallel series, so as to concentrate their action more strictly into the

Hence it appears that the direct induction is vastly greater than the influence communicated to the remote end of the bar, being nearly five-sixths greater at the fourth length, about twice as great at the third length, about thrice as great at the second length, and about quadruple at the fourth length.

The relative force of the inductive influence under the first arrangement,—that is, as developed at the farthest end of the bar, has been shown to be in the ratio of 1, $\frac{1}{4}$th, $\frac{1}{9}$th, $\frac{1}{14}$th, nearly, at the respective distances of 1, 2, 3 and 4 focal lengths of the bar and magnet. But, under the latter arrangement, in which the magnetism is directly induced on the same end of the bar as that whose attractive energy is measured, a different ratio is obtained. Taking the first tangent-difference of the first series at 100, the relative energy of induction at the other distances successively become as 29.0, 8.6 and 4.5, which, expressed fractionally, exhibit this series, in which the first is unity, $\frac{1}{1}$, $^{1}\frac{0}{3}$, $\frac{1}{12}$, $\frac{1}{22}$ nearly. But, calling the first tangent of the second series (110104) 100, the ratio becomes 100, 20, 5.3, or, fractionally, as $\frac{1}{1}$, $\frac{1}{5}$, $\frac{1}{20}$ nearly.

These results, exhibiting such considerable differences, may be considered perhaps rather as perplexing than satisfactory. They might seem to intimate, that, where the law of attraction is so simple, the experiments not according with this law must be erroneous. But simple as the law as to a single, separate attraction is, the estimation of the variety of influences which come into account, in these investigations, is far from being so easy as at first sight it may appear. For, whilst the power of the nearest pole tends to develope in the nearer end of the iron-bar a magnetism contrary to its own, the tendency of the remotest pole of the magnet, being of a contrary polarity, is, in the inverse proportion of the square of its distance, to alter the magnetism developed by the other pole. But this is not all. For each of the poles of the magnet have, at the same time, their direct influence, however diminished in energy by increase of distance, upon the remote end of the bar, which influence is

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east and west line of the compass. Still, however, the smallest error of observation or position, where the angle is so great, necessarily produces very considerable differences in the tangents, so as to render the observation of the nearest position unsatisfactory.

necessarily transmitted in modification of the direct action of the magnet on the nearest extremity of the bar. These different forces, indeed, may be easily calculated, but the manner in which they act reciprocally under the different arrangements remains to be determined.

Notwithstanding what has now been stated, the results derived from the foregoing experiments may be considered, perhaps, as so far satisfactory, as affording a comparison of the quantity of magnetism directly induced upon the nearer end of a bar, and of that developed at the remote extremity, and indicating also the proportion of resistance in the iron itself to the perfect transmission of the inductive energy from end to end.

Another mode of experimenting, however, which subsequently occurred to me, is much more practical, satisfactory and useful, affording, by direct experiment, the actual quantity of magnetism induced into any iron or steel bar or bars of different capacity, in reference to the energy of the inducing magnet itself, together with the exact proportion of influence produced on each at any given distance.

The arrangement, in this case, which was peculiarly simple, required only this adaptation, that the bars of iron or steel used for trying the quantity of induction should be of the same length, or nearly of the same length, as the magnet employed. And when comparative experiments were made on the relative capacities of different kinds of iron and steel, or iron and steel of different tempers or degrees of hardness, it further required (in order to obtain at once direct and final results) that each bar should be of the *same* dimensions as the magnet made use of.

Fig. 5. Plate II. represents this new arrangement. M is a magnet placed in the direction of the east or west point of the compass C at *any* distance,—say at two *focal* lengths from the centre of the compass, when the deviation by either pole is carefully observed. An iron bar, I, of the same dimensions as the magnet, is then placed exactly over it, separated by two little blocks of wood, or other substances of equal thickness, adapted to the distance required for the experiment. As the magnetism now induced through the whole length of the iron bar is of the opposite kind to that of the inductive magnet, its action on the compass must also be opposite; the deviation is consequently found to be diminished, and that (measuring by the tangents)

exactly in the proportion which the magnetism induced in the iron bears to the actual energy of the magnet itself. The relation of the induced magnetism with that of the inductive energy being thus at one distance simply and experimentally ascertained, the proportion of induction due to any other distance is as easily determinable.

On this plan a series of experiments on the power of induction with a bar of soft iron of the same dimensions as the magnet, was made at a variety of distances from contact up to five inches, together with two comparative series, at the same distances, with two other soft iron-bars of similar length and breadth, but differing in thickness, one being $\frac{1}{16}$th of an inch thick, and the other an inch, whilst the magnet itself was ¼th of an inch thick.

The following Table exhibits the mean deviation of both ends of each iron-bar, in juxtaposition with the magnet, as placed at 17 different distances, the magnet, which was 13¾ inches long, being uniformly at the distance of 24 inches from the centre of the compass, where, by its sole action, it produced a deviation of 18° 14′.

Table of the Effects of Induced Magnetism on three different Bars of Soft Iron, at various distances.

Distance of Iron Bar above the Magnet.	Square Bar, 1 Inch by 1 Inch.		Flat Bar, 1 Inch by ¼.		Iron Hoop, 1 Inch by $\frac{1}{16}$.	
	Mean Deviation of both ends.	Diff. of action of Bar and Magnet alone, or 18° 14′.	Mean Deviation.	Diff. of action of Bar and Magnet alone, or 18° 14′.	Mean Deviation.	Diff. of action of Bar and Magnet alone, or 18° 14′.
Inches.	° ′	° ′	° ′	° ′	° ′	° ′
5	16.54	1.20	17.38	0.36	17.53	0.21
4	16.17	1.57	17.16	0.58	17.36	0.38
3	15.27	2.47	16.51	1.23	17.8	1.6
2¾	15.12	3.2	16.34	1.40	16.54	1.20
2½	14.55	3.19	16.21	1.53	16.47	1.27
2¼	14.27	3.47	16.3	2.11	16.18	1.56
2	13.57	4.17	15.40	2.34	15.51	2.23
1¾	13.30	4.44	15.15	2.59	15.25	2.49
1½	12.52	5.22	14.43	3.31	14.59	3.15
1¼	12.13	6.1	14.8	4.6	14.19	3.55
1	11.24	6.50	13.29	4.45	13.39	4.35
¾	10.27	7.47	12.35	5.39	12.49	5.25
½	8.53	9.21	11.15	6.59	11.32	6.42
¼	6.54	11.20	9.21	8.53	9.42	8.32
⅛	5.22	12.52	7.59	10.15	8.38	9.36
$\frac{1}{16}$	3.49	14.25	6.38	11.36	7.45	10.29
Imperfect Contact.	3.0	15.14	5.32	12.32	6.32	11.42

Hence the ratio of influence on the soft iron-bar $\frac{1}{4}$ of an inch thick (being precisely of similar dimensions to those of the magnet) was as follows, nearly: At the distance of 5 inches, the iron-bar acquired $\frac{1}{50}$th of the power of the magnet; at 4 inches distance the inductive influence of the magnet, was $\frac{1}{19}$th of its own power; at 3 inches $\frac{1}{13}$th; at 2 inches $\frac{1}{7}$th; at 1 inch $\frac{1}{4}$th; at $\frac{1}{4}$th of an inch $\frac{1}{2}$; and at $\frac{1}{16}$th of an inch $\frac{2}{3}$.

But the proportional influences at the various distances will be more evident, by a reference to the annexed additional Table of *deductions*, from the middle series of experiments made with the quarter-inch bar, which was similar in all its dimensions to the magnet.

Deductions from Experiments with the bar of soft iron $13\frac{3}{4}$ inches long by 1 inch broad and $\frac{1}{4}$ inch thick, and Magnet of same size.

Distance of iron-bar above the magnet.	Mean Deviation.	Tangent.	Diff. of deviations of magnet and magnetic-bar, 18.14.	Diff. of Tangents, or Tangent deviation of Magnet and Bar, —32943.	Proportionate force of Induction.
Inches.					
5	17.38	31786	0°.36′	1157	12.9
4	17.16	31083	0.58	1860	20.7
3	16.51	30287	1.23	2656	29.6
2	15.40	28046	2.34	4897	54.6
1	13.29	23977	4.45	8966	100
3	16.51	30287	1.23	2656	16.1
$2\frac{3}{4}$	16.34	29748	1.40	3195	19.4
$2\frac{1}{2}$	16.21	29337	1.53	3606	21.9
$2\frac{1}{4}$	16. 3	28769	2.11	4174	25.3
2	15.40	28046	2.34	4897	29.7
$1\frac{3}{4}$	15.15	27263	2.59	5680	34.4
$1\frac{1}{2}$	14.43	26266	3.31	6677	40.5
$1\frac{1}{4}$	14. 8	25180	4. 6	7763	47.1
1	13.29	23977	4.45	8966	54.4
$0\frac{3}{4}$	12.35	22322	5.39	10621	64.5
$0\frac{1}{2}$	11.15	19891	6.59	13052	79.2
$0\frac{1}{4}$	9.21	16465	8.53	16478	100
$0\frac{1}{8}$	7.59	14024	10.15	18919	
$\frac{1}{16}$	6.38	11629	11.36	21314	
Contact,	5.32	9688	12.32	23255	

The last column of this table represents the proportionate force of the induced magnetism at the several distances of 1, 2, 3, 4, and 5 inches; the force at the distance of an inch being called 100.

In this method of determining the proportions of the magnetism induced, both with reference to the power of the magnet made use of, and with respect to the distance of the bar from the magnet, it was assumed that these proportions would be the same at whatever distance from the magnet the compass employed for the deviations might be placed. In order to verify this assumption, as well as to acertain the degree of consistency to be expected in similar experiments, elsewhere made, another series of deviations, under a similar arrangement, and with a magnet and an iron-bar of the same dimensions as the former, was observed some months after the foregoing deductions had been calculated. The distance of the bars from the compass was, in this instance, changed—being now 12 inches instead of two feet.

The following were the deviations and results obtained from this series, as to the proportions of induced magnetism at the different distances of 1, 2, 3, 4, and 5 inches; the magnet alone producing a deviation of 61° 15′.

Distance of Iron-bar above the Magnet.	Mean Deviation of both ends.	Tangent of Deviation.	Dif. of Deviation of Magnet and Bar together, and 61° 15′.	Dif. of Tangents, or Tangent-Deviation of Magnet and Bar — 182276.	Proportionate Force of Induction.	Proportionate Force of Induction by former Experiments.
Inches. 1	52° 5′	128379	9° 10′	53897	100.0	100.0
2	56.57	153693	4.18	28583	53.0	54.6
3	58.47	165011	2.28	17265	32.0	29.6
4	59.45	171473	1.30	10803	20.0	20.7
5	60.15	174964	1. 0	7312	13.5	12.9

Comparing the last two columns, we have a coincidence sufficiently near to verify the assumed principles,—that the measure of the actual, as well as proportionate forces of induced magnetism, is correctly derived from the tangents of the deviations, and that the proportions are not dependent on, or influenced by, the distance at which the compass is placed from the *magnet* and iron-bar. The differences, indeed, in the ratio of induction, in reference to distance, are only such as may reasonably be ascribed to the errors in the measurements of the distance betwixt the magnet and the iron-bar—these distances not having been adjusted with that attention to perfect accuracy which an absolute coincidence would have required.

Various as the preceding modes of examining the inductive power of the magnet are, and simple and consistent as the results of the last method have proved, yet we do not find that the law of magnetic induction as to distance is satisfactorily shown by any of them. Whilst the law itself is unquestionably uniform and consistent, the effects of that law are found to vary according to the mode of directing the inductive energy. When the influence is that of a magnet upon a bar of iron lying in the same straight line, the induced energy, as estimated by the tangents of deviation of the compass, approximates the inverse ratio of the squares of the distance; but when the influence is that derived from the whole mass of the magnet into the whole mass of the bar, lying in parallel juxtaposition, then the ratio appears to be totally different. For when, in the latter case, measures of an inch are made the integer of distance, then the ratio of energy becomes $\frac{1}{1}$, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{5}$, and $\frac{1}{8}$ nearly. But when the *thickness* of the magnet, or of the bar (both being the same), is made the integer of distance, then, the energy at $\frac{1}{4}$ of an inch interval, the first distance, being called 100, the series becomes 100; 79.2; 64.5; 54.4; 47.1; 40.5; 34.4; 29.7; 25.3; 21.9; 19.4; and 16.1,—at the 12th interval.

Unsatisfactory as some of these investigations may appear to be, I have been encouraged to proceed thus far with the subject—not only because of the interest necessarily belonging to any of the laws with which the wisdom of God has endowed the various forms of created elements; but because, likewise, of an important practical application of the influence under consideration, to which it appears to be peculiarly adapted. I refer to the employment of magnetism of induction for estimating the *quality* of iron, as to the strength or ductility of which we have no satisfactory test but that of positive trial.

The ductility of iron being that property which most chiefly determines its value in commerce and manufactures, it is as a measure of this quality that the magnetic influence, I consider, may be applied. For there seems to be a particular relation between the ductility of iron, and its capacity for magnetism—the softest and most ductile iron having the greatest capacity for the magnetic influence. Hence it is considered, that the experimental determination of the capacity of different specimens of iron by the method adopted in the last experiment, might (as already suggested) afford a simple and decided test and measure of their relative quality and value in commerce.

For this purpose a bar-magnet of moderate dimensions (say of 12 inches in length) and a small compass would be all the apparatus requisite. The different specimens of iron, whose proportionate quality as to ductility is to be determined, might require to be forged into the shape and size of the magnet, or at any rate into a similar shape and size with each other, and then softened equally by being moderately heated and allowed to cool slowly in the same place, or under similar circumstances. The magnet being then placed in the east or west direction of the compass, say at two lengths distance, will indicate, by the deviation of the needle, its own power of attraction, which, being observed, one of the specimens of iron is laid exactly over the top of the magnet separated by two small blocks of wood, or other substance of equal thickness, and then the deviation produced by both extremities of the iron-bar alternately observed. (See Fig. 5.) The difference between the mean of these deviations and that of the magnet alone, will serve as a measure of the capacity of that particular specimen for induced magnetism. Whilst the compass and magnet remain undisturbed, the rest of the specimens can be brought successively to the test, being always kept at equal distances from the magnet by the same interposed substances, when the comparison of the measures of their different capacities for induced magnetism will, if the theory be correct, afford a certain indication of their relative qualities.

With a view of assisting me in verifying this theory, my friend Edward Roscoe, Esq. of Liverpool, kindly furnished me with several characteristic specimens of the different qualities of iron most commonly manufactured in Britain. Though the examination of these did not enable me to discriminate small differences, yet, when the *common* and *best* qualities were compared, the magnetic capacity of the latter proved to be nearly one-tenth *greater than* any of the common kinds.

CHAP. II.—EXPERIMENTS ILLUSTRATIVE OF THE NATURE, INFLUENCE, AND PHENOMENA OF MAGNETIC INDUCTION.

SECT. 1. *Of the Nature of Magnetic Induction.*

MAGNETIC Induction, some of the laws of which have now been examined, is of most extensive influence in the science of magnetism, being more or less engaged as an agent in almost all its phenomena. The attraction of ferruginous bodies, not having previous polarity, by the magnet, depends simply on their inductive capacity,—the proximate end of the magnet first developing a polarity different from its own, and then, according to the general law of both magnetism and electricity, attracting the contrary polarity thus developed. In whatever substance, therefore, magnetism can be induced, there will be a capability of being attracted by the magnet, the degree of attraction being proportionate to the inductive capacity. It is also the general influence by which polarity is developed in bodies capable of permanent magnetism.

Though we have no means of explaining the essential nature of an agent so subtle and extraordinary as that of magnetism, yet we may exhibit, by striking experiments, a variety of its properties and phenomena. The connection of the magnetic principle with iron is, as we have said, inherent and inalienable, and the two denominations or polarities appear to have permanent residence in each ferruginous particle, being mobile in the particles but not separable from them.

Ferruginous bodies are usually and naturally devoid of any strong magnetic energy, the arrangement of the magnetisms of the different particles being such as in a great measure to neutralize each other. But the mere proximity of a magnet, or even an electrical influence, as has been shown, disturbs the natural equilibrium of the polarities, and tends to arrange them by its inductive energy in a magnetic series.

The development of the magnetic condition in a bar of iron, then, is the mere arrangement of the inherent polarities in a magnetic series, which arrangement appears to be the result of two influences,—the direct inductive influence of the proximate magnet on each of the particles of the iron, and the communicative influence of the various magnetic particles in the bar upon each other. Hence the energy with which the inductive influence on the end of the bar nearest to the magnet is exhibited at the other.

Of these two influences, this experiment seems to afford a satisfactory illustration.

Experiment 1.—*Illustration of the neutral condition of Iron, and of the Direct and Communicated influence of induction.*

A bar magnet, 12 inches in length, being placed in the direction of the west point of a small compass, at the distance of a foot from its centre, produced a deviation in the needle of 39°.40′. Between the magnet and the compass I now placed, in the manner represented in Pl. II. fig. 6. *a*, a series of six similar pieces of iron-wire, previously softened in the fire, 1.7 inches in length each, and $\frac{1}{10}$th in diameter. The wires in this arrangement being placed at right angles across the straight line joining the magnet and compass, derived no polarity, because of their position, and, consequently, could exhibit no influence; the deviation, as expected, was therefore unaltered. Each piece of wire being now turned on its centre, so as to preserve its relative distance, was next arranged without contact, as in series *b*, and then the deviation was 43°.41′, being an increase through the magnetism developed in the wires of 4°.1′. The pieces of wire were, lastly, placed in contact in a straight line, as in series *c*, each piece being at the same distance both from the magnet and compass as before, when the deviation was found to be 49°.47′, being a farther increase, occasioned by the transmitted influence, of 6°.6′.

Suppose, now, these pieces of wire to represent separate magnetic particles, and they will be found very well to illustrate the principal phenomena of induction. Under the parallel form of Fig. 6. *a*, we have a representation of an unmagnetised bar of iron, in which the particles, under the influence of their inherent spontaneous attractions, are mutually neutralized. Under the next form (*b*), we have an illustration of the proportion of direct inductive influence of the proximate magnet, on all the separate particles in the aggregate. And under the last form, of a continuous line in contact (*c*), we have an experimental example of the increase of magnetic energy, produced by the inductive influence of the particles upon one another. And if we compare the two influences in the case before us, the relative magnetic energies being proportionate to the tangents of the angles of deviation in the compass-needle, we find that the magnetism induced on the separate particles directly by the magnet, was to that induced by the particles upon one another as 2 to 3 nearly.

This relation, however, is found to vary in different ferruginous bodies, and in the same kind of iron or steel of different degrees of hardness, the transmitted induction, or the influence of particle upon particle, being constantly smaller as the hardness of the metal increases. This fact, which was first suggested speculatively, by the consideration of the nature of the phenomena, was afterwards verified as to iron and steel of three different states of hardness, by repeated experiments, of which the following is an example.

Experiment 2.—*Examination and illustration of the proportions of directly induced and communicated magnetism in pieces of soft iron, wire-drawn steel, and hard steel.*

The pieces of the different descriptions of metal consisted of six in number of each kind, all very nearly of the same dimensions as the wires made use of in the preceding experiment. The magnet was placed about 12 inches from the compass, and $\frac{1}{20}$th of an inch from the end of the nearest wire; the series extending 10 inches, brought the other extremity within 2 inches of the centre of the compass. Each series of wires was then successively interposed between the magnet and compass, at equal intervals from each, and the deviations, in the two arrangements *b* and *c* of Fig. 6. observed. As, however, the different series of wires were not *accurately* of the same length, the distance of the magnet from the compass became subject to a small alteration; but the effects of this on the results is not material.

The following are the results of the three series of wires.

SERIES A.

Inductive influence on the SOFT IRON-WIRES.

The action of the magnet alone was a deviation of 40°.45′.

Out of contact (as Fig. 6 *b*)	44°.13′ — 40°.45′ = 3°.28
In contact (Fig. 6 *c*)	51.45 — 40.45 = 11.0
Hence the influence transmitted from bar to bar, -	51.45 — 44.13 = 7.32

SERIES B.

Inductive influence on the WIRE-DRAWN STEEL.

The action of the magnet alone was in this series 40°.26′.

Deviation out of contact,	42°. 7′ — 40°.26′ = 1°.41′
——— in contact,	43.19 — 40.26 = 2.53
Hence the influence transmitted from bar to bar, -	43.19 — 42. 7 = 1.12

SERIES C.

Inductive influence on the HARDENED STEEL WIRES.

Here the action of the magnet alone was a deviation of 39°.52′.

Deviation out of contact,	40°.47′ — 39°.52′ = 0°.55′
——— in contact,	41.20 — 39.52 = 1.28
Hence influence from bar to bar,	41.20 — 40.47 = 0.33

Whilst, therefore, in the series of iron-wires, the total inductive influences, *direct and transmitted*, were productive of a deviation of 11°.0′, in augmentation of the unaided deviation of the magnet; the influences in the wire-drawn steel occasioned but 2°.53′ augmentation, and in the hardened steel only 1°.28′.

And whilst the *direct* induction on the iron-wire occasioned an augmentation of 3°.28′, that on the wire-drawn steel was 1°.41′, and on the hardened steel 0°.55′.

And whilst the *transmitted* influence produced in the iron-wires a power of augmentation of 7°.32′ tc the deviation occasioned by the magnet alone, the wire-drawn steel produced but 1°.12′ augmentation, and the hardened steel wires only 0°.33′.

Hence the comparative capacities for induced magnetism of the three series, as estimated by the differences of deviation, are as follows:

	1st Series.	2d Series.	3d Series.
Direct influence,	3°.28′	1°.41′	0°.55′
Transmitted influence,	7.32	1.12	0.33
Total inductive energy,	11.0	2.53	1.28

The proportion of energy of the magnetism directly induced on the different portions, and that transmitted from portion to portion, it must be observed, affords no measure of the relation between the two influences in an actual bar of iron, because there the number of the particles, and the intimacy of their connection, renders the transmitted energy by far more considerable in soft iron than the direct influence.

SECT. 2.—*On the Inductive Effects of the Magnet on Iron.*

The experiments detailed in the preceding parts of this essay are all illustrative of these effects; but, being designed for the examination of the laws and nature of induction, they have been limited in their character to that particular class which seemed the best calculated for the attainment of these objects. This section of the essay, however, not being confined to any particular investigation, will admit of all such original experiments as may serve to illustrate any of the various influences and phenomena of this interesting property of the magnet.

SERIES A.

With a single bar-magnet, or with two or more equal bars placed over each other in parallel juxtaposition, and with similar poles coincident.

As these experiments are the more striking, in proportion to the power of the magnets, it is advisable, if the bars be well tempered, so as not to injure each other by their proximity, to employ at least a pair of magnets, according to the arrangement of fig. 1. before given. The original experiments were made with a pair of three-feet magnets: but nearly the whole series can be exhibited, though with less striking effects, with a pair of good twelve-inch bars.

Experiment 1.—*For the Suspension of Iron Balls.*

These balls consist of the best soft iron, and require to be turned in a lathe and well polished. A convenient size is $\frac{4}{10}$ths of an inch in diameter, weighing about 70 grains. Of this description sixty to eighty balls will be requisite, and from twenty to thirty, of smaller dimensions, in a graduated series.

The suspension of the balls in a single dependent chain of connexion, is a satisfactory means of trying the power of the magnets. A pair of good twelve-inch magnets will suspend about six equal balls of seventy grains weight,—perhaps eight or nine, if gradually reduced in size towards the bottom; whilst my three-feet magnets will sustain no less than fifteen equal balls, or eighteen diminishing in size, in a single series. Here we have a beautiful illustration of the inductive power of the magnet, assisted by the inductive influence of each ball upon the one contiguous to it in the series. The polarity of the first of the chain is developed directly by the action of the magnets, but the second is at once influenced by the first ball and by the magnet, and so on throughout the series.

The balls, when employed without any other apparatus, may be attached by the magnetic influence in a number of pleasing forms, as festoons, fringes, and bunches of grapes.

But I proceed to describe some more novel arrangements.

Experiment 2.—*For the Suspension of Balls at the extremities of Crooked Wires.*

(1.)—Fig. 7. represents a modification of the experiment with the balls, which has a very pleasing effect. For this experiment two pieces of iron-wire, about 3 inches long, and $\frac{1}{8}$th in diameter, bent into the form represented at *b*, and notched into each other, are added. Being crossed at the notched part, and attached to the pole of the three-feet magnets, each extremity of the wires will sustain eleven or twelve equal balls. The substance between the wires and the magnets is an oblong piece of polished iron, by the interposition of which the effect of the experiment is improved, though the number of balls capable of being suspended is diminished. It may be necessary to observe, once for all, that the points of contact of all the substances employed in these experiments should be highly polished, and, to obtain the best effects, the iron of which they are composed should be annealed or softened previous to the polishing.

(2)—Fig. 8. *a*, Pl. III. exhibits another modification of the experiment, in which a half link of iron-wire, *b*, similar to the former in thickness and quality, is appended to each extremity of the crossed wires, and the balls are now suspended from every point. Each of these, *without* the interposed iron, will sustain from two to ten or more balls, according to the power of the magnets.

(3.)—An elegant variety in this experiment is obtained, under powerful magnets, by adding to the half links suspended from the cross wires, a second half link to the extremity of each of the first series; and, even at this distance from the magnets, a chain of six graduated balls (see Fig. 9.), may be suspended from each of the sixteen downward terminations of the last series of wires.

But these three last experiments admit, of course, of an unlimited variety of modifications.

Experiment 3.—*Formation of Chains of Half Links.*

A pair of good twelve-inch magnets will sustain a chain of about six half links, of the size of wire above described, which may be increased to five full links, measuring above nine inches in length, by the use of the three-feet magnets. Fig. 10, *a*.

This experiment is modified, as represented at *b*, by the interposition of a ball between each complete link; at *c*, by a ball at each limb of the half links; at *d*, by the introduction of the crooked wire, Fig. 7. *b*, &c.; and at *e*, by a weight of cork suspended from the fourth complete link.

Experiment 4.—*For Suspension, without contact, of Small Masses of Iron.*

This is a very striking and curious experiment. Fig. 11. *a*, represents the general arrangement. A very little key held down to the table by a piece of fine thread, or a hair, is brought within a small distance of the extremity of the magnets, where, the thread being on the stretch, it can be suspended without contact, quivering in the air. With twelve-inch magnets, the distance at which the key can be sustained will be exceedingly small; but with three-feet magnets it may be supported more than a quarter of an inch below them, so as to admit of plates of glass, metal, or thin wood, to be passed between. A key, weighing 580 grains, has been supported by my own three-feet magnets without contact.

If two keys or wires be suspended contiguously at the same time, they will repel each other, and if a small magnet be brought near them they may be attracted, repelled, or agitated, to a considerable limit.

Modifications of the experiment are represented at *b*, *c*, *d*, *e*, *f*. A small piece of tinned iron is suspended at *b*; at *c*, two pieces of iron-wire; at *d* a crooked piece of wire *; at *e* a small kite covered with silver paper, with a piece of wire in the middle; at *f* a balloon, formed out of an egg-shell, reduced to extreme thinness in vinegar, and coloured. An axis of iron-wire yields the requisite suspensive energy.

The story of the suspension of Mahomet's coffin within a loadstone cave is amusingly illustrated by this experiment.

SERIES B.

With a compound horse-shoe magnet and an iron-bar, or single bar-magnet, for partially neutralizing one of its poles.

All the experiments of the preceding series can be equally well performed with a good compound horse-shoe magnet, according to the arrangement represented in Pl. IV. Fig. 12.

The magnet being suspended from a frame of wood, is so adjusted in height that one of its poles should rest upon a large flat bar of iron, or else upon the end of a bar-magnet with opposite poles coincident. By this means the pole in contact with the flat bar is partially neutralized, so that the operation of the unattached pole is left more free and unembarrassed for the performance of the experiments.

It is not necessary to repeat here the series of experiments with this apparatus, being precisely similar to those under the preceding arrangement.

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* This wire, weighing 18 grains, was suspended by the pair of three-feet bars, at the distance of three-eighths of an inch below. In performing this experiment, in any of its modifications, it will be found advantageous to employ a very short thread, otherwise its elasticity will prove inconvenient, if not fatal, to the success of the experiment.

SERIES C.

With two equal bar-magnets, placed over each other at a few inches distance, with *opposite* poles contiguous.

As in all the experiments of the first series, the magnetism induced on the different masses of iron was chiefly upon the portions nearest to the magnets, the influence on the opposite extremity of each portion being merely consequential, it occurred to me, that if the substances employed in the experiments were placed betwixt the two opposite poles of a pair of magnets, so that the appropriate polarity might simultaneously be developed at both extremities of the iron, a much more powerful action might be expected. In this expectation I was not disappointed; for, adopting the arrangement represented in Pl. IV. Fig. 13, I found that experiment No. 4. could now be accomplished at distances considerably greater, and experiment No. 2. with a greater number of balls, whilst a variety of new illustrations of the phenomena of magnetic induction were successively suggested, as I proceeded with the amusing investigation.

Experiment 1.—*For the vertical support of Nails and Wires on their points.*

Iron-wires (*a*, *b*, Pl. IV. Fig. 14.), placed vertically on their pointed extremity, can be supported by the large magnets at the distance of two inches from the upper magnet, and the nail *c*, weighing 326 grains, at the distance of an inch and a half from it.

Experiment 2.—*Small Figures in paper, or card board, vertically supported.*

A pleasing variety is produced in the foregoing experiment, by attaching the iron-wires to little figures cut out of paper (Pl. IV. Fig. 15.), which, standing upright betwixt the magnets, and quivering on the points of the wire, are made to vibrate, or whirl round, in mimic life, by approaching them with another magnet, and waving it around them. These figures, three inches in height, can be readily sustained at the distance of an inch from the upper magnet, the magnets themselves being four inches apart.

Experiment 3.—*For the support of small Wires and Nails upon each other's extremities or points.*

With the large bars, four inches apart, iron-pins (the common black pins of the shops) will stand on each other two inches below the upper magnet, or three or more in a vertical series, firmly adhering to each other, and yet vibrating freely on their supporting point, whenever a small magnet or piece of iron is brought near them. *a*, *b*, *c*, Pl. IV. Fig. 16. represent different series of pins; *d* a key supported on the point of a nail, and *e*, two polished nails.

Experiment 4.—*For the support of different articles in an upright series.*

The variety of amusing series capable of being sustained under this arrangement is evidently unlimited. The forms and groups represented in Pl. IV. Fig. 17. may serve as specimens of what may be accomplished, with the articles only previously in use. The form represented at *a* is produced by a nail, with two half links of iron-wire suspended on its point. *b* is a half link of iron on the point of a nail supporting four balls near the upper magnet. The next form *c* represents six half links or a chain of three full links, sustained vertically. *d* consists of three balls upon a half link. *e* exhibits three balls upon the point of a nail. *f* represents a nail of two inches on its point, which will not stand alone; but when the head is set round with black pins, though the heads of these are nearly an inch from the upper magnet, the whole is freely sustained. The form represented at *g* consists of a two-inch nail, on the point of which is an iron ball, and that ball set round with black pins, like the plume of the thistle.

Additional Observations on the Relation of Nitric and Nitrous Acids to Bromine and Iodine. By ARTHUR CONNELL, Esq. F.R.S.E., &c. Communicated by the Author.

IN a late notice, mention was made of some unsuccessful attempts to oxidate bromine by means of nitric acid, by a process similar to that which effected the conversion of iodine into iodic acid *. It was farther stated, that when a small quantity of bromine was boiled with nitric acid for a considerable time in a long tube, the upper and open extremity of which was bent and terminated in water, and the intermediate part kept cool by moistened bibulous paper, so as to condense the bromine as it sublimed, and cause it to fall back into the acid, the water, after the free bromine which had passed over was expelled by heat, gave with nitrate of silver a pretty plentiful precipitate of bromide of silver. This precipitate I have since found, from an examination of the liquid in a variety of ways, was caused by the presence of hydrobromic acid; and the question arose, to what cause the occurrence of that acid could be attributed. To ascertain whether bromine was capable of decomposing pure water by similar treatment, and of so giving rise to the formation of hydrobromic acid, a little bromine was boiled with water under the same circumstances; but after the liquids employed had been deprived of colour by a gentle heat, no hydrobromic acid could be discovered by the agency of chlorine, and subsequent agitation with ether. It became necessary, therefore, to look for some third body, which, by its affinity for oxygen, might contribute to the decomposition of water; and as a coloured and fuming nitric acid had been employed, it appeared probable that nitrous acid might have that effect. Accordingly, on making the experiment with colourless nitric acid and bromine, and afterwards driving off free bromine from the water, in which the extremity of the apparatus terminated, by a gentle heat, no hydrobromic acid could be observed in it, when examined by means of chlorine and ether. On the other hand, when the experiment was repeated with a red and highly fuming nitric acid, the presence of hydrobromic acid in the water was made abundantly manifest by the same reagents. A like result was obtained, when an acid was employed which had been highly charged with nitrous acid, by passing through it a current of deutoxide of azote.

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* This Journal, April 1832.

From these experiments, it seems to follow that, under the influence of bromine and nitrous acid, a portion of water is decomposed by long continued boiling, hydrobromic acid, and probably nitric acid, being formed. This result is somewhat remarkable, because, under ordinary circumstances, nitric and hydrobromic acids mutually decompose one another. It does not appear that all the bromine employed becomes hydrobromic acid, a part of it being usually volatilized.

As it would appear that the precautions necessary for diminishing the loss of iodine in the preparation of iodic acid by the action of nitric acid have not been well understood [*], I think it proper to add the following particulars, willingly leaving to others the determination of the comparative merits of the different methods which have been proposed for the preparation of iodic acid.

The essence of the method by nitric acid, consists in using a vessel of very large capacity in relation to the quantity of materials employed. I would recommend that it should be capable of containing forty or fifty times the quantity of nitric acid actually used. The reasons obviously are, to afford a large internal surface on which the iodine volatilized may be condensed, and from which it may be washed back again into the acid, and to diminish the quantity of acid-vapour escaping by the neck, which ought to be as narrow as possible [†]. A very strong acid ought also to be employed, and the boiling is best maintained by the small flame of a spirit-lamp, so as to prevent, as much as possible, the heating of the sides of the vessel. Operating in this way, although I have always experienced some loss of iodine, yet I do not conceive that, with due care, the loss is so great as to constitute a serious objection to the method *. There are few chemical processes which are not attended with some sacrifice of materials; and it seems a matter of very little consequence on what material that loss falls, provided the total expense, in comparison with other methods, is not increased. Indeed, plans might easily be devised, by which all the iodine which escapes oxidation might be condensed, and saved for another operation. And if it be objected that the process of ebullition is tedious, I shall leave to others to determine whether more time and trouble are expended, than in the numerous steps of other methods.

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[*] See Annales de Chimie et de Physique, xlix. 144; and Annal. der Phys. xxiv. 363.

[†] It is very convenient in washing back the iodine, to be able occasionally to fit in a glass stopper, so that the liquid may reach every part of the vessel, without danger of being spilt.

It will give me much pleasure, if the suggestion of M. Serullas, whose recent loss science deplores, shall be found to facilitate the process; but in the only experiment which I have made on the subject, I could not observe that the employment of an acid, which had been highly charged with nitrous acid, by passing through it a current of deutoxide of azote, offered greater advantages.

Major-General Sir Howard Douglas, Bart. &c. on Military Bridges, and the Passage of Rivers in Military Operations†.

THE passage from one point of a country to another, with facility, will always, among a commercial and warlike people, form a topic of general interest for discussion. In ancient times, one of the greatest obstacles to the free intercourse of one nation with another, and of different parts of the same kingdom with each other, was generally understood to arise from the badness of the roads, and the want of bridges on large and rapid rivers. To this cause may, in some measure, be attributed the slow progress of civilization, of the cultivation of the arts and sciences, and of the general diffusion of useful knowledge. A conqueror did occasionally arise, inflamed with ambition, and actuated by an enthusiasm for conquest, who carried in his army men possessing a knowledge of all the science of the age in which they lived. When a Greek or Roman general had conquered a kingdom or a province, the first care of a conqueror generally was, to introduce among the inhabitants a knowledge of the arts then known; and to this cause many of the ancient European nations owe the introduction of the first rays of intelligence by which they were enlightened. To retain possession of conquests likely to be recovered by former rulers from intestine commotions, or loss by the sudden incursions of neighbouring tribes, a ready communication from one place to another, and a rapid march of troops to quell any occasional insurrection, or hostile attack, would form the first objects of the conqueror's solicitude. Hence arose the Roman roads all over Europe, and particularly in Britain, where traces of their remains yet traverse the island from one extremity nearly to the other. If, in the course of these roads, a large river crossed its direction, which in floods was impassable by troops, then a magnificent bridge was generally erected. There were thus constructed numerous bridges in the course of a Roman itinerary. These have been of great advantage, even in modern times, by affording facilities of communication that would otherwise have been unattainable. The methods of selecting lines, and of laying out of roads, have been lately much improved; therefore, the points where formerly bridges had been erected, have been found to be injudicious; and, consequently, in countries where manufactures and commerce have been introduced, it has been found indispensable to change the line of direction, and to select new sites for bridges; though, in those countries that have slightly changed their condition for centuries, the old Roman bridges have continued to be used with considerable advantage, both for the purposes of commerce and of war.

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* I speak here of the result of my own experiments, which were not conducted with any particular view to economy; but I observe that M. Duflos, in following out these experiments, has succeeded in converting half an ounce of iodine into iodic acid, by means of 2½ ounces of nitric acid, *without any loss of iodine at all.—Bullet. des Sciences*, Oct. 1831.

† This article is to be considered as illustrative of the highly interesting and very important work, entitled, "An Essay on the Principles and Construction of Military Bridges, and the Passage of Rivers in Military Operations." By Major-General Sir Howard Douglas, Bart. &c. Second edition, pp. 417.

Next to the commodious situation of a bridge, its judicious construction forms an important consideration. Arches and domes were used in buildings long before their properties were investigated on scientific principles. The invention of the arch is frequently attributed to the Greeks, as it has been discovered in several of the most ancient temples of Greece, but nothing of the kind is to be found in any of the ancient monuments of Persia or Egypt. Most of the apartments of the ruins of ancient Egypt are covered with a single stone; and, in the galleries of the pyramids, of which the roofs consist of numerous pieces, their peculiar construction renders it highly probable that the builder was ignorant of the arch. The Greeks seem, therefore, entitled to the honour of the invention. The arched dome, however, appears to have been invented by the Romans. In the later monuments of Italy, the Etruscan dome, and the Grecian temple are combined, as in the famous Pantheon, even in its most ancient form.

In modern times, mathematical and mechanical principles have been applied to the investigation of the properties of bridges. Dr Hook proposed the inverted catenary as the best form of an arch, in which all the strains would balance one another. This curve may be applied to most cases that occur, though it has seldom or never been used in practice. The circular arch has been, from the simplicity of its construction, most generally introduced. Of late years, however, the elliptical arch, from its convenience in keeping the crown of the bridge low to suit the level of the road, and its superior elegance, has been much employed. When the semiellipse forms the arch, this curve rises more rapidly at the haunches than the semicircular, of the same span, and thus, besides the advantage of being lower at the crown, the under part of the arch forming the voussoirs, is by that means much more capacious, and consequently better fitted to admit a free passage of the water in high floods.

Permanent bridges have generally been constructed of stone, sometimes of wood; and, for temporary purposes, of boats, especially in military operations, which require them to be speedily erected, and as quickly removed. Suspension bridges have been long used in India and America; and of late years they have been introduced in Europe, both for civil and military purposes, with considerable advantage. These methods should be all carefully studied, their respective merits well considered, and their applications most minutely scrutinized, by those professionally engaged in their construction.

Besides a knowledge of the scientific principles on which bridges are constructed, an acquaintance with the general laws observed by fluids in motion is of much practical advantage to the civil and military engineer. By a proper application of these laws, the most advantageous position for a bridge may be selected, and its permanence secured. On this account Sir Howard Douglas commences his work properly with a section on the principles and effects of the motion of water in rivers. He justly remarks:

> "Although it is not the object of this work, to consider the doctrine of the motion of water in canals and rivers, in relation to the purposes of civil life, yet so many deductions, highly important to our present subject, may be made from it, that a few observations upon hydraulics will be found a useful introduction to the various methods of passing rivers in military operations.
>
> "A knowledge of the principles of the motion and action of water, enables us to trace that mode of unceasing operation which occasions sinuosities; regulates the velocity of the current; forms eddies, and consequently banks; determines the natures of the sections at different parts of rivers, and many other points connected with their local circumstances, essential to the proper application, construction, and security of military bridges, and to the calculation of the effects or the delivery of water, whether for inundation, subsistence, or force."—Pages 1 and 2.

Sir Howard Douglas then notices the imperfect theories of Guglielmini and Varignon. The latter became proverbial among the academicians at Paris, for a predilection to generalizations, without a sufficiently careful appeal to experiments; and even Belidor, who has been considered one of the most profound of scientific engineers, adopts the same theories in his *Architecture Hydraulique*. The applications of these theories, however, to the courses of the rivers Po and the Danube, showed their imperfections; and the Abbe Bossut and the Chevalier Du Buat were by that means induced to undertake each, an extensive and varied series of experiments in hydrodynamics, for the express purpose of improving these theories. The former was himself an eminent mathematician, and applied his extensive knowledge of that science very successfully to the subject. The latter was assisted by a talented young officer of engineers, M. Benzech de St Honoré, who had been appointed his colleague in their conjoint experimental course. The whole forms an instructive specimen of the method of discovering the laws of nature, involved in a series of complicated phenomena. The Chevalier Du Buat's first experiments were undertaken to establish the similarity of the motion of water in pipes and open canals. From these he proceeds to show that the moving principle of water arises from gravity and the mobility of the particles, which causes the fluid to assume a level in close vessels, or determines it to move to that side where there is a defect of pressure; and, excepting in very small tubes, there is scarcely any declivity so small that water will not move in it,—are all owing to the same cause. From this reasoning Du Buat derives his first principle, namely, *that the motive force of each particle of water composing a river arises simply from the slope of the surface.*

Again, if a current of water were not resisted by the bed in which it runs, and if its fluidity were perfect, its velocity would become more and more accelerated, to such a degree that its destructive impetuosity would become irresistible. But the friction of the water against the bed of the river, which, by the effect of adhesion, is communicated to the whole mass of fluid, causes a resistance, which, augmenting nearly as the squares of the velocity, at length equals the accelerating force, when the velocity will remain uniform, without the possibility of increasing, unless a change takes place in the slope of the river, or in the dimensions of the section.

From the foregoing reasoning, Du Buat obtains his second principle, namely, *that when a stream moves uniformly, the resistance is equal to the accelerating force.*

It follows from observation, combined with reasoning, on the nature of rivers, that, in open canals and rivers, the greatest velocity is at the surface in the middle of the stream, and in close pipes in the central line or axis; because there the moving water is most remote from all causes of resistance. If this velocity, therefore, be taken to calculate the force or discharge of the water, the results will, in both cases, be erroneous in excess. The situation of the line or stream of mean velocity in the section varies with so many circumstances, that it cannot be precisely ascertained; though, by a series of very ingenious experiments performed by Du Buat, the following laws were discovered:

1. In small velocities, the velocity at the surface exceeds that at the bottom in a very considerable ratio.

2. This ratio diminishes in proportion as the velocity of the current increases; and, in very great velocities, approaches nearly to a ratio of equality.

3. Neither the magnitude of the bed, nor the slope of the river, changes this proportion when the mean velocity remains the same.

4. When the velocity at the surface is constant, that at the bottom is constant also, whatever be the depth of water, or the magnitude of the section.

5. The mean velocity is an arithmetical mean between that at the surface and that at the bottom.

It is very difficult to determine the velocity at the bottom by experiment; and the proportion between it and the former diminishes as the mean velocity increases. Du Buat determined this variation by experiment, and thence deduced the following rule. To find the bottom and mean velocities when that at the surface is known, take unity from the square root of the surface velocity, expressed in inches, and the square of the remainder is the velocity at the bottom. Half the sum of these two will give the mean velocity.

Ex. If the surface velocity in the middle of the current, be 25 inches per second, what is the bottom and mean velocity? *Ans.* $\sqrt{25} - 1 = 5 - 1 = 4$, and $4^2 = 16$ the bottom velocity; therefore, $\frac{25 + 16}{2} = 20\frac{1}{2}$ the mean velocity. As the mean velocity is most generally required, it may be found by the following rule. *From the observed surface velocity, in the middle of the stream, increased by* $\frac{1}{2}$, *subtract the square root of the surface velocity, the remainder will be the mean velocity of the whole section.* From the same example as before, $25\frac{1}{2} - \sqrt{25} = 25\frac{1}{2} - 5 = 20\frac{1}{2}$, the mean velocity obtained more readily than by the former rule.

This discovery is of great importance, because the velocity of the surface, in the middle of the stream, is easily measured, from which, by this rule, the mean will be readily obtained; and it is the mean velocity that must be used in calculating the discharge, supply, force, and every other effect of running water.

Several experiments are resorted to for the purpose of determining the mean velocity of running water, such as a cylindrical rod of wood, loaded at the lower extremity with a piece of lead, or straight glass tubes, having their bottoms filled with small shot, to keep them perpendicularly in the water, and at such a depth as may suit the depth of the stream. In absence of these, weeds or shrubs, having a proper quantity of earth adhering to their roots, may be conveniently substituted, when great precision is not required. By using a sufficient quantity of these at different points in the breadth of a stream, the mean velocity of the whole flowing mass of water may be obtained with considerable accuracy. By observing the mean time in seconds by a good watch, or half or quarter seconds pendulum, the volume of water that flows through a given space or distance in a given time, may be accurately computed, which may be turned to considerable advantage in many philosophical inquiries*.

From a vast mass of experiments, Du Buat proceeds to deduce an empirical formula, on the principles that have now been detailed, and at last arrives at the following expression for *the mean velocity of running water, in French inches, per second of time.*

$$V = \frac{297(\surd r - 0.1)}{\sqrt{b} - \text{hyp. log} \times \sqrt{b + 1.6}} - 0.3(\surd r - 0.1) \quad . \quad (1.)$$

In which V denotes the *mean* velocity of the water in French inches, r the radius of the section, and b the distance, divided by the fall in the same measure, that is, if the fall be two feet in a mile, then $\frac{2}{5280} = \frac{1}{2640} = \frac{1}{b}$, and therefore $b = 2640$.

By r, or the radius of the section, is meant the area of the transverse section of the river in square inches, divided by the linear surface of the water in contact with its banks. Or it may be defined to be the quotient obtained by dividing the area of the transverse section of the stream expressed in square inches, by the boundary or perimeter of that section, diminished by the breadth of the upper surface of the stream, in linear inches. To those not very familiar with algebraical formulæ, it is rather difficult to apply formula (1.) to practical purposes, and therefore attempts have been made to simplify it for the use of practical men, without sacrificing much of its accuracy. For this purpose, Robison, in his Works on Rivers, vol. ii. page 444, adopts the second form of Du Buat, who, in p. 63, reduces it to English inches, and it becomes

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* In Dr Thomas Thomson's Treatise on Heat and Electricity, page 268, a comparison is made between the fall of rain, and quantity of evaporation, by means of the discharge on the Clyde. Though the principle is good, yet we doubt the accuracy of *his* result.

$$V = \frac{307\,(\sqrt{d} - 0\,.\,1)}{\sqrt{b} - \text{hyp. log.} \times \sqrt{b + 1\,.\,6}} - 0\,.\,3\,(\sqrt{d} - 0\,.\,1) \quad . \quad (2.)$$

from which he deduces a table that is easily applied. Sir Howard Douglas proceeds to a farther simplification, and in a note, p. 17, adopts

$$V = \frac{306\,.\,7\sqrt{r}}{\sqrt{b}} = 306\,.\,7\sqrt{\frac{r}{b}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3.)$$

This, though less accurate, will be sufficient for many practical purposes.

In page 20, we meet with some very interesting and useful remarks, relative to the action of water on the beds of rivers selected from Du Buat. This gentleman found—

> "by experiment, that the greatest velocities which the various substances expressed below can resist without moving, are

		Velocity at bottom, per second.
Fine sand,		6 inches.
Coarse, angular, rough sand,		8
Gravel,	Fine, the size of grain of anniseed,	4
	Mean, the size of a pea,	7
	Coarse, the size of a bean,	12
Pebbles, an inch in diameter,		24
Angular stones, the size of an egg,		36

> "These results shew the velocities required to cleanse or deepen canals of any kind from any deposit, or accumulations of either of the substances given above; and for military purposes, it may be of service to remark, that this furnishes useful data to enable us to judge of the consistency of the bottom, which it is not always possible to do experimentally, by merely measuring the velocity of the current at the surface, and then referring to the

rule (page 14) for that at the bottom, or determining the mean velocity with a rod. From this we may form some estimate what a decrease of velocity may have in rendering the bed more foul; or an increase in the velocity of the current in removing soft banks formed of substances actually deposited. For rivers may, after floods, become passable, where they were before too muddy to be forded; or a decrease of velocity may occasion a deposit of mud and slime, where it was before clean. Rivers which are not any where fordable perpendicularly across, may sometimes be found passable in a slanting direction between two sinuosities. In searching for fords, therefore, when, from the size of the river, their existence is doubtful, the trial should be made somewhere between any two sinuosities, not far distant from each other."

To perform this, it may in general be remarked, that the bank should be first entered at about one-third the length of the *reach*, supposed moderate, not exceeding half a mile or a mile, below the first sinuosity or convex turn of the bank, and ought to be turned slanting downwards towards the opposite convex bank, as much above it as the entrance was below the first.

" Thus crossing obliquely the transit of the current from side to side, both extremes will be avoided, and I have frequently found rivers fordable in this manner, which could not be crossed at any point perpendicularly. The Spanish army with which I served forded the Esla, in the campaign of 1812, without loss or difficulty, by taking advantage of this circumstance; and in the same manner I forded the Douro near Zamora, and several other formidable rivers."

The author terminates the first section with many similar judicious remarks on the passage of rivers.

The second section of the work is devoted to the properties of the pontoon, and to the manner of laying a pontoon bridge. A pontoon, it is well known, is a portable boat, conveyed on an appropriate carriage from one place to another, along with an army. When a sufficient number of these boats, with their appurtenances, can be carried with a given number of soldiers, they serve the purpose of constructing a bridge over any unfordable river in the course of their march. In the British service, the pontoons are generally made of tin, supported by a proper frame. There are two kinds usually made for our service. The larger-sized pontoon weighs about 9 cwt. The appurtenances, constituting the flooring, &c. for the bridge, consisting of beams, strong deals, boards, oars, an anchor, a grapnel, bolts, lines, pickets, &c. weigh about 13 cwt.; and the four-wheeled carriage upon which each pontoon is transported weighs also about 13 cwt.; making the whole 35 cwt. The smaller, with its appurtenances, weighs only about 27 cwt*.

The author gives accurate practical rules for determining the depths to which either of the pontoons in our service will sink in water when loaded with a given weight, and also conversely, to find the weight of load corresponding to a given depth. From these, a table is added, showing at once, by inspection, the depths corresponding to given weights, which must prove very useful to persons not conversant with intricate calculations, or when an estimate of these must be made quickly for any special purpose. The method of using this table (page 38), is exemplified by applications to infantry, cavalry and guns.

> "The comparative powers of the two kinds of pontoons," the author adds, (page 43), "are exhibited in the preceding pages; from which it appears that, with the same weight of infantry, the small pontoon is immersed 4 inches more than the other; and not being so deep by 3½ inches, the gunwales are then only 6¼ inches above the water. Perhaps the adoption of a medium size, as the only nature of a pontoon, would be preferable."

The author then states the equipment of a full train of 36 pontoons, in four divisions, but which our limits will not admit us to subjoin.

In page 48, the method of laying a pontoon bridge is very fully explained, and methods of determining the distance of the opposite banks of a river clearly illustrated. The author throws the investigation of some of his practical rules into notes at the bottom of the page where they occur, and by this does not interrupt the narrative. It would have been an advantage to have given a formula and rule for the trigonometrical operation also, though this may, in his opinion, be supposed to be derived from works on trigonometry. In some of these that are really adapted to practical men, a convenient rule or formula may be found, though most of our ordinary treatises usually taught in schools are remarkably deficient in these particulars. The following rule would have been convenient for this purpose.

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* To render these safe from the effects of shot piercing their sides, it would be very convenient to have the interior cavity subdivided by tin traverses, so that, though even one or two of the intermediate spaces were filled with water, still the pontoon would not sink. To prevent the pontoons filling with water, a tarpauling or canvass deck might be placed on such of them as were *most liable to* such accidents.

To the logarithmic sines of the measured angles A, B, Plate I. Fig. 15, add the logarithmic cosecant of their *sum*, if, as in the figure referred to, the perpendicular CD falls *within* the triangle, or the logarithmic cosecant of their difference if it falls without, and the logarithm of the measured base AB; the sum of these, rejecting tens in the index, will be the logarithm of CD, the perpendicular breadth of the river. We have been thus minute in this particular, because, if the breadth of a river is not accurately known over which a pontoon bridge is to be thrown, the capture of a place, or the loss of an army, may be the consequence.

"This was abundantly proved in the passage of the Garonne at Grenade, in 1814, by the army of the Duke of Wellington, previous to the battle of Toulouse. That city served the enemy as a double *tête de pont.* Its ancient walls were in a very defencible state, on both banks of the river; and the communication between these two works was kept up by a good stone-bridge. The inhabitants of the country had been employed, for some time previous to this period, in repairing and strengthening the ancient ramparts, and in mounting the cannon on them. Two attempts had been made, a few days before that at Grenade, to pass the river above Toulouse. The first, at a place called Portel, failed for want of a sufficint number of pontoons to reach across; there being still about 26 yards of river remaining after they were all placed in the line. This failure arose from not having correctly ascertained the width of the river at the place recommended for effecting the passage."

This shows in a remarkable manner the necessity of determining the breadth of a river with precision.

"The second attempt was made at St Roques, a town situated on the enemy's bank. The operation began at dusk. A party of about fifty infantry were rowed across, and they barricaded themselves in a building favourably placed for protecting the operation. The bridge was immediately commenced, finished before day-light, and the troops passed; but the roads by which they were to operate were not found to be in a practicable state, on account of the previous rains, and the troops were ordered to return to the left bank."

It was next determined to pass the river below the city. The bottom here, as well as at St Roques, proved gravelly, and none of the anchors would hold. The cables next the left bank were, therefore, secured to trees; those next the right bank, at the suggestion of the Duke of Wellington, were made fast to anchors buried on shore. These, and many other interesting topics relative to the passage of rivers by means of pontoon bridges, are detailed with great minuteness, which cannot fail to be useful to all engineers engaged in similar pursuits.

The third section treats of Bridges of Boats. The subject is managed with great ability. The French engineers think that the size of the boats should correspond with the magnitude of the river. For the Danube, the length should be 60 feet; for the Po, 50 feet; for the Tanaro or Taro about 20 feet, with a breadth of about 10 feet and a depth of 3 feet. Sir H. Douglas is of opinion, that the two first are far too long. In large tide rivers, subject to heavy swells, decked lighters are very convenient; and in some cases, as in the passage of the Adour by the Duke of Wellington's army in 1814, they were indispensably necessary. A curious, and we should think useful, composition for covering the tarred canvass applied to pontoons, may be seen at pages 105 and 106.

After having shown the most approved methods of laying down pontoon bridges, the manœuvre of withdrawing a bridge entire is also carefully described, and every operation correctly explained. The converse operation of restoring a bridge after it has been withdrawn, in both small and large rivers, is treated at full length, so as to be easily understood by engineers employed in these services. Our author next illustrates his different maxims by detailing the passages of various rivers by distinguished military commanders.

> "The passage of the Douro, in May 1809, by Lieutenant-General Sir Arthur Wellesley, may be cited as a splendid and very instructive example of what may be effected by judicious combinations and arrangements, with very trifling material means, in forcing the passage of a river not properly watched and guarded; and shows the importance of providing columns of troops intended for such enterprises by a few row boats, with which to commence instantly an attempt that might otherwise be defeated by the most obvious and simple precautions on the part of an enemy,—a practical deduction may well be enforced from the fact, that this very celebrated operation was commenced with one small boat, procured by an accidental circumstance. The whole of this operation is beautifully described by Colonel Napier in his account of the Peninsular War.
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> "After the affair of Grijon on 11th of May, in which the French troops were forced to abandon their position, and to retire, pressed by the British cavalry, first to Carvalho and then to Villa Nova, opposite to Oporto, they crossed the Douro by the bridge of boats, and, in the night, broke up that communication.
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> "The reports made to Marshal Soult of the direction of General Hill's

march, having led the Marshal to believe that the passage of the river would be attempted below Oporto, he turned his attention more particularly to the lower part of the river on account of the difficulty or impossibility of effecting a passage opposite to or above Oporto. Wellesley perceiving that Soult had not adopted sufficient precautions for observing the river, and from preventing a passage from being forced above Oporto, determined to make the attempt there. Never was plan laid with more consummate ability,—never operation executed with greater intelligence and determination. A small boat, which, by the flight of an individual from Oporto in the night, had accidentally escaped the general seizure, having been discovered, Colonel Waters gallantly made use of her to pass unperceived in quest of others, and soon returned with three or four large barges. In the mean time, a battery of eighteen or twenty pieces of ordnance was established on the height of Sierra, which forms a salient, extremely favourable for forcing a passage there: some light troops were sent higher up the river to search for boats, and a considerable force marched towards Avintas, to effect a passage at the salient part of the river near that place.

"Early in the morning of the 12th, some troops of Lieutenant-General Sir Edward Paget's division were pushed across the river, from the upper part of the sinuosity formed by the Sierra point, and consequently concealed from the town; and that gallant officer took possession of a large building, forming a seminary nearly opposite. The French, completely surprised, made the greatest exertions to recover that important post; but the first battalion of the Buffs, supported by the 48th and 66th regiments, under Major-General Hill, (who, on Sir Edward Paget being disabled by the severe wounds which he received on this brilliant occasion, succeeded to the command of these troops), maintained their position with the most determined gallantry, notwithstanding the repeated attacks made on them by large bodies of troops under Soult in person.

"Some troops of General Sherbrooke's division now began to pass in boats which were recovered from the broken bridge; and General Murray crossed at the adjoining salient near Avintas. The passage being thus effected, the French columns were obliged to move in haste, which was soon pressed into confusion, along the Valonga road, under a destructive fire from three batteries under General Hill, now posted in the seminary enclosures; and if it had been possible to make the movement from Avintas somewhat earlier, Soult's retreat by that road would have been intercepted, and the French army ruined."

We have been tempted to make this long quotation on account of the instructive lesson which it gives on military strategy, when the operations are conducted by men of science and talent; and, for the benefit of the young engineer, we imagine we cannot do better than give the very able and judicious remarks of the learned author.

"The defeat as well as the victory of this day are full of instruction on the precautions of surveillance and other measures, which should be taken to preclude surprize, and to prevent a passage, by open force, from being effected; and on the measures and means most conducive to the success of enterprises of this description. Soult did not adopt proper measures of surveillance. No arrangements were made for a rapid communication of intelligence, by signals, by which means only that prompt concentration of troops on the points of attack can be effected, on which the defence of a river-bank mainly depends. The plan of operation on the offensive part was justly conceived, well combined, and most gallantly executed. The quick perception of Soult's error, in believing himself secure, and of his neglect of those precautionary measures which he ought to have adopted, were admirably acted upon. The Seminary proved an excellent *tête de communication;* the batteries placed on the convent height commanded the ground in front of the *tête,* so that, when once gained, the footing on the right bank could not well be lost; and the movement from Avintas was judiciously calculated to conceal from the enemy an important operation, and to reap the fullest fruits of a victory which all these combinations richly deserved."

Such, then, are the spendid achievements of a brave army, well disciplined, provided with engineer officers of intrepidity and skill, when commanded by a consummate general. Such also are the disasters of an army, however brave, when under the direction of an improvident leader.

The fourth section of the work is occupied with the methods of constructing Flying Bridges, and employing them for the passage of troops across unfordable rivers. Sir Howard Douglas gives several of the most convenient and effective modes of erecting the necessary apparatus for this purpose. The flying-bridge is formed by anchoring a floating body, such as a buoy in or near the centre of a river, so that a boat attached to it by a cable or chain may receive the action of the stream obliquely. By this means a force is derived from the current, which moves the vessel across the river. The manœuvre will be more easily executed with a somewhat long cable than with a short one. In great rivers, such as the Rhine or the Danube, two large boats or vessels are commonly used. On the sides of these, parapets of timber, wool, sand-bags, &c. may be constructed, as was practised by Charles XII. in his passage of the Duna in 1701. The author, after having giving complete information on the methods of constructing and managing flying-bridges, illustrates the whole with many interesting and appropriate examples, which are of great utility to the practical engineer and commanding officer.

These are chiefly drawn from the campaigns of Napoleon, the Arch-Duke Charles of Austria, and the Duke of Wellington, for which we must refer to the work itself.

The fifth section contains a dissertation on Bridges or Rafts of Timber, Casks, Air-tight Cases, and Inflated Skins. In addition to the method of constructing rafts, a variety of useful rules for finding the contents of beams, and valuable tables relative to their contents and specific gravity, are subjoined. An article on guaging, or determining the capacity of casks, in order to obtain their buoyancy, is likewise recorded. Inflated bags, made of the skins of animals, were even suggested by the Rhodian to Xenophon, to enable him to pass the Tigris; and Alexander had recourse to this method in order to pass the Oxus. Livy tells us that some of Hannibal's Spanish infantry passed the Rhone by swimming, with the assistance of inflated leathern bags, and Cæsar informs us, that the Spanish light infantry practised this method of passing rivers, as is at this day employed on the Tigris and Euphrates. Canvass-bags, lackered over with a solution of caoutchouc (Indian rubber), have lately been proposed, and in India, basket-boats, covered with undressed hides, are often employed. In short, the extent and variety of information relative to these more temporary expedients can only be duly appreciated by those employed in such operations, to whom the examples will prove invaluable.

Our attention is next directed in section sixth to Carriage-Bridges and Suspension-Bridges. Carriage-bridges are supported on four-wheeled carriages, and are found to be very useful in expeditiously forming temporary bridges in small shallow rivers and canals. Sir Howard does not recommend the *Pont-volant* of the *Aide Mémoire*, a French book similar to his own, but there is no doubt that the ordinary carts and waggons of a country may be sometimes found very useful for such purposes. A carriage-bridge for infantry, two abreast, was made across the Douro, between Tordesillas and Toro with the spring-waggons of the army. The bottom of the river was hard and even; the average depth being from three to four feet. The waggons were placed longitudinally, at distances suited to the lengths of the planks that had been collected for flooring, which were laid from waggon to waggon, the tail and front boards being taken out. The same thing may be done with the

-wheeled carts, by merely laying the ends of the each cart into the body of that in front of it.

ed author next enters into a minute, clear, and full ll the circumstances attending the construction of usi ridges.

arned note, at pages 307, 308, and 309, the author the equation to the catenary, both in a complete and in iate form.

be the depression of the middle of the chain below the ontal line, called generally by mathematicians the absciss; f the horizontal distance of the points of suspension, or nd z half the length of the curve; then, if the weight of a unit of the chain, such as one foot, be w, the weight of half the chain will be $w\,z$, and $w\,c$ and $w\,t$ will be the tensions at the middle of the chain, and at each point of suspension. From these considerations, and the properties of the suspended chain, Sir Howard arrives at his equation (6); or,

$$c = \frac{3\,y^2 + x^2}{6\,x} = \frac{y^2}{2\,x} + \frac{x}{6} \quad . \quad . \quad . \quad . \quad (6)$$

$$\text{and, } z = y + \frac{2\,x^2}{y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

To these we shall add those given by Professor Sir John Leslie, from which every thing necessary to the construction of suspension-bridges in all ordinary cases may be derived with the greatest simplicity *. Taking the distance between the points of suspension equal to d, the depression of the middle of the chain below the horizontal line equal to h, the length of the chain equal to l, and the strain at the middle of the chain, or parameter of the curve, equal to p; then, in all practical applications where the depression is small compared with the length of the chain, the

$$\text{strain at bottom, or } p = \frac{d^2}{8h} + \frac{1}{6}\,h = \frac{l^2}{8h} - \tfrac{1}{3}\,h \quad . \quad . \quad (8)$$

$$\text{strains at points of suspension, or } s, = \frac{d^2}{8h} + \frac{7}{6}\,h \quad . \quad . \quad (9)$$

$$\text{or, } s = \frac{l^2}{8h} + \tfrac{1}{3}\,h \quad . \quad . \quad (10)$$

$$\text{and } l = d + \frac{8\,h^2}{3\,d} \quad . \quad . \quad . \quad (11)$$

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[*] See Elements of Natural Philosophy.

Whence it appears, that Sir John has, by these simplifications of complex formulæ, rendered an important service to practical engineers. Sir H. Douglas applies these formulæ, or rules derived from them, to the construction of a rope bridge, where the span is 130 feet, the depression 12 feet, the weight of the bridge unloaded 12,490 lb., and when loaded with infantry 34,090 lb. From which—

1. The whole length of the curve,	= 132.95	feet.
2. Weight of one foot of the bridge unloaded,	93.94	lb.
3. Loaded,	256.41	...
4. Strain at the lowest point unloaded,	16726	...
5. Loaded,	45652	...
6. The greatest strain, or that at the highest point, unloaded,	17853	...
7. Loaded,	48729	...
8. The angle which the chain makes with the vertical, at the highest point,	69° 32′	

Now, it appears, from tables given by Sir H. Douglas and others, that—

A rope, 3 inches in diameter, or 9.42 in circumference, will sustain a weight of	27350	lb.
A rope of 6 inches in circumference,	11088	...
Hence six floor cables will sustain	164100	...
And the two suspension cables,	22176	...

These are the tensions which the ropes can safely support *without risk;* and, consequently, the floor cables alone have abundant strength to sustain all the strains to which they may be subject.

The author then adds, that—

"One of the principal arches of Trajan's Bridge[*], across the Tagus at Alcantara, having been destroyed by the French, Lord Wellington found it necessary to direct that a communication across that bridge should be re-established, for the purpose of bringing up artillery and stores from Badajos, for the attack of the forts at Salamanca. Timber of sufficient dimensions to effect this could not be easily procured, and indeed any application of that material to make good such a fracture would have been extremely difficult, and required much labour to be performed on the spot, in fashioning, framing, and setting up the work; and which, consequently, would have given warning to the enemy, before the campaign opened, of some important movement in that quarter being intended. To obviate these difficulties and objections, (Lieutenant-Colonel Sturgeon, of the Royal Staff corps), the officer sent in

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[*] An elegant plate of this bridge, repaired as here described, forms a beautiful frontispiece to the title-page of this work.

> April 1812 to make preparations for this operation,—a man of fertile genius and great practical knowledge,—happily devised an application of cordage, which might be prepared secretly, and even in privacy, at any distance from the place at which it was to be used, and easily transported thither entire, and speedily stretched across whenever it might be required. The broken arch left void was about 100 feet wide, and between its shattered buttresses a precipitous chasm 140 feet deep. Across this abyss the slender material spread gracefully but securely. A work so slight and temporary, contrasted strangely with a massive structure which had stood for ages; but the happy expedient made good, in a few hours, a way over ruins of vast account; and formed an auspicious opening to the important scenes that were now about to be acted on the great theatre of war."

The seventh and last section treats of Bridges, on Trestles, Piles, Truss-frames, and other applications of Carpentry. This section is managed with the same ability as the other parts of the volume, and cannot fail to be highly useful and instructive to military engineers, though, from the length to which this article has already extended, we are reluctantly forced to dispense with an analysis of its contents. To this is subjoined a valuable appendix, on the strength of timber and other materials, of great importance to all descriptions of practical men, of either the civil or military professions.

We now terminate our examination of this most useful and interesting volume, with Segur's graphic description of the passage of the Beresina by the French army under Napoleon, in his disastrous retreat from Russia in 1812; and the author's judicious reflections on the propriety of keeping up, even in peace, a corps of pontoneers.

> "When the Imperial Guard began to move, the stragglers dispersed in the surrounding woods and villages, who had not taken advantage of the first night to cross the river, now rushed from all sides, and flocked to the river side in one dense and confused mass, which soon choked up the narrow entrances to the bridges. The foremost, impelled by those who followed, were driven upon the guards and pontoneers, who were endeavouring to keep a passage open for our troops. In repressing these crowds of fugitives, many were trodden under foot in the mêlée, others thrown upon the floating ice, and great numbers, unable to regain the bridge or to reach the shore, perished in the river. The efforts of Napoleon and his officers to re-establish order were unavailing. So great was the confusion, that force was necessary to clear a passage even for the Emperor, who crossed at about 2 P. M., with nearly 6000 of the guard under Ney. The troops continued to pass till the morning of the 29th; but multitudes of the stragglers, benumbed with cold, were unable to avail themselves of this last opportunity; and about 8 in the morning, General Eblé, seeing the Russians advancing, was obliged to set

fire to the bridges, leaving vast quantities of ammunition, artillery, and baggage,—thousands of men, and many women and children, to the mercy of the enemy, and to the rigours of a merciless climate.

"Napoleon is said to have exclaimed, when he heard that Tchaplitz (the Russian General) had abandoned his position—'Ah! J'ai trompé l'Amiral;' and certain it is, that if Tchitchakof had not descended the Beresina, directing Tchaplitz to move in that direction likewise, upon the supposition that Napoleon would not attempt the passage above, his further retreat would have been intercepted there. The escape of Napoleon was thus so much owing to the foresight, ability, and enterprise of General Eblé, and to the intelligence and experience of the corps of pontoneers, by which Napoleon was enabled to profit promptly by this mistake, that these details cannot be too forcibly cited, as proof of the vast importance of our keeping up, during peace, the nucleus at least of a corps, which cannot be formed upon an emergency. The multifarious matter contained in this book all appertains to its duties, and may show, that if, by inconsiderate economy, we reduce the establishments in which only these executive qualifications can be perfected, we shall unquestionably, some time or other, have again to encounter difficulties, such as those which were experienced in all the early operations of the late war. It is a sage maxim in state economy, that, in proportion as constitutional, political, or financial, circumstances render it expedient to reduce the numerical force of its army during peace, it becomes the more necessary to maintain the establishments in which practical science is upheld; and no one, I think, who reflects upon the very extensive, important, and elaborate attributes of the corps upon which such services as these depend, can doubt the expediency of preserving such a nucleus of it as may insure the retention at little cost, of what has been acquired in a long series of arduous and expensive training on actual service."

To these striking and forcible remarks, which carry conviction to every mind capable of just reflection, little need be added by us to recommend the protection of our seminaries to public notice. If we allow our scientific institutions, either naval or military, to be injured or suppressed, we may venture to predict that the country will pay dearly for the sacrifice. Euler and Bouguer were honoured and rewarded for their theoretical investigations of the principles which should be observed in shipbuilding and naval equipments, and their efforts have been long apparent in the superior qualities of the vessels in the French marine. Before the establishment of our naval and military colleges, their engineers were equally superior to ours, though they were perhaps inferior towards the close of the late war. It remains with the government of the country to determine what must be our future status as engineers in either service.

Experiments on the Expansion and Contraction of Building Stones, by variations of Temperature. By WILLIAM H. C. BARTLETT, Lieutenant United States Engineers [*].

SIR, *Fort-Adams, Newport Harbour, March* 12. 1832.

IN the progress of this work, we have had occasion to use considerable quantities of coping-stones taken from different localities, with all of which it has been found impossible to obtain tight joints. The walls on which these stones were placed have not undergone the slightest change; and notwithstanding they were laid with the greatest possible care, and their joints were filled with the best cements that could be devised, yet, at the expiration of a few weeks, these joints were broken up by fissures which extended from the top to the bottom of the coping. These fissures were supposed to have arisen from a change of dimensions in the coping-stones, in consequence of the ordinary variations of atmospheric temperature; and, with the view to ascertain if the total amount of cracking could be attributed to this cause alone, a series of experiments was instituted by order of Colonel Totten, and continued from 18th August 1830 to 2d June 1831. The circumstances connected with these experiments, as well as their results, you will find subjoined. Colonel Totten requests me to communicate them to you, supposing that you may find them of sufficient practical importance to deserve a place in your Journal.

These experiments were made nearly at the same time, upon granite, limestone, and sandstone, the kinds of stone used for the coping; and for this purpose a piece of each was selected, in such a manner that the three pieces were of nearly equal lengths. The granite has a fine grain, is of a compact texture, and was taken from a boulder at the head of Buzzard's Bay: the limestone is white, has a fine grained crystalline structure, and accompanies primitive rocks; it was taken from the quarries of the Sing-sing State Prison, New York: the sandstone is from the quarries in Chatham, Connecticut, and belongs to the old red sandstone formation, according to the Rev. Edward Hitchcock; [†]

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[*] Silliman's Journal, Vol. xxii. No. 1. April 1832.

[†] He now refers all the sandstone of the Connecticut valley to the new red sandstone.

it has a granular structure, rather coarse, and its cement is argillo-ferruginous.

To ascertain the exact lengths of these pieces at the different temperatures produced by exposure to the weather, these alone being important for our immediate object, and for the purposes of construction generally, the measurements were made by means of a white fine rod, with copper elbows at the ends, embracing the stones when applied to them, as represented in the sketch.

Fig. 1

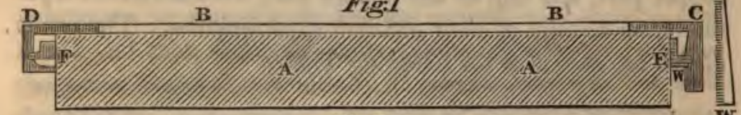
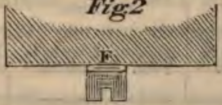


Fig. 2



AA is an elevation or vertical section lengthwise of the stone to be measured; BB the measuring rod, with elbows D and C, of thin hammered copper, firmly secured to it. The end D was always adjusted to the same part of the stone, by sliding through a groove in the copper guide F cemented to the stone; the elbow C was adjusted in like manner by sliding through a groove in the piece E, also attached to the stone. The elbow C has itself a groove, through which the wedge W may slide horizontally under the guide E, between the elbow C and the stone. This wedge being graduated as a diagonal scale, showed by the distance which it entered, the difference between the length of the measuring rod and that of the stone. The expansion of the measuring rod being known, the length of the stone could be calculated in decimals, viz. the English standard inch.

A groove was cut in the stone, in which a thermometer was placed at each measurement, and, being covered, was suffered to lie some time, in order to ascertain the temperature of the stone. The temperature of the measuring rod was assumed to be that of the open air to which it had been exposed.

By Lardner and Kater's Mechanics, we have, as a mean between the results of Captain Kater and Dr Struve, for the linear expansion of deal wood, in terms of its length, for one degree of Fahrenheit, the decimal .00000255; and by the Edinburgh Encyclopædia, article Expansion, we find the decimal .00000944 to express the same for hammered copper. From these data, the actual length of the measuring rod was calculated for each experiment, knowing its length at 60° Fahrenheit. But to abridge the calculation, the difference in length between the stone and measuring rod, as shown by the wedge W, was substracted from the length of the rod, before making the reduction for the temperature of the latter. The length of the copper part, and that of the wooden part, were calculated separately, on account of their different expansibilities. The result of this calculation is the following Table.

	MARBLE.		GRANITE.		SANDSTONE.	
No. of Experiment.	Degrees Fahr.	Lengths in inches.	Degrees Fahr.	Lengths in inches.	Degrees Fahr.	Lengths in inches.
1	6°	93.4155	6°	94.0251	6°	94.0180
2	7	93.4277	8	94.0330	8	94.0153
3	9	93.4201	9	94.0260	9	94.0052
4	10	93.4207	10	94.0265	10	94.0088
5	11	93.4131	11	94.0230	11	94.0124
6	12	93.4186	12	94.0282	13	94.0211
7	14	93.4174	14	94.0271	14	94.0206
8	14	93.4294	14	94.0347	14	94.0220
9	14	93.4308	14	94.0361	15	94.0235
10	16	93.4302	16	94.0285	15	94.0238
11	16	93.4291	16	94.0345	17	94.0214
12	17	93.4305	17	94.0358	18	94.0181
13	19	93.4327	19	94.0416	20	94.0239
14	20	93.4310	20	94.0364	22	94.0258
15	21	93.4316	21	94.0440	22	94.0263
16	31	93.4265	32	94.0324	32	94.0371
17	32	93.4352	32	94.0406	34	94.0466
18	34	93.4422	36	94.0330	38	94.0554
19	36	93.4360	36	94.0450	39	94.0436
20	36	93.4357	37	94.0483	39	94.0592
21	38	93.4436	41	94.0344	43	94.0486
22	52	93.4323	52	94.0348	53	94.0560
23	58	93.4450	62	94.0541	64	94.0718
24	83	93.4655	86	94.0720	93	94.0879
25	86	93.4649	88	94.0737	93	94.0829
26	90	93.4709	88	94.0688	95	94.0897
27	99	93.4677	89	94.0731	99	94.0941
28			90	94.0693	100	94.0906
29			91	94.0693	101	94.0944
30			94	94.0628	104	94.0841
31			102	94.0721	109	94.0792

It is probable that many of the discrepancies here noticed were owing to the hygrometric state of the stone, and perhaps in part to imperfections in the measuring apparatus; but as the hygrometric state of the stone was not recorded, we can take no account of it in our deductions. These discrepancies, however, will have but little effect upon the general result, for it will be observed that there is always an increase in the length of stone for an increase of temperature, when any two experiments are considered which are removed from each other by several degrees.

From the facts ascertained concerning the expansion of other substances, we may assume that the expansion of stone is uniform; and that, within the range of our experiments, each of the stones increased in length by a common difference for each degree of the thermometer. To find an approximate value for this common difference, say for the granite, we substract the first observed length from the last, and, if these experiments were accurate, the difference .0470 would be ninety-six times the common difference, ninety-six being the difference in degrees between the extreme temperatures: the same operation being performed with the second experiment, and that next the last; the difference .0298 (the difference in lengths) should be eighty-six times the common difference. By thus comparing the extreme experiments of those which remain, we obtain the following Table.

Experiments.	Diff. in Degrees.	Diff. in Lengths.
1 and 31	96	+ .0470
2 ... 30	86	+ .0298
3 ... 29	82	+ .0433
4 ... 28	80	+ .0428
5 ... 27	78	+ .0501
6 ... 26	76	+ .0406
7 ... 25	74	+ .0466
8 ... 24	72	+ .0373
9 ... 23	48	+ .0180
10 ... 22	36	+ .0063
11 ... 21	25	— .0001
12 ... 20	20	+ .0125
13 ... 19	17	+ .0034
14 ... 18	16	— .0034
15 ... 17	11	— .0034
Total .	817	.3708

We have neglected the sixteenth experiment, because we cannot employ it without using some other experiment twice, thus giving the latter an undue influence, and because the middle term should have the least weight in determining the common difference.

By the above table, we find, as the combined result of all the experiments, that .3708 should be 817 times the common difference; and hence the common difference for one degree of Fahrenheit is .0004538 inch. Now, assuming 94.05 inches as the mean length of the granite, which is sufficiently near, we find the linear expansion for one inch of stone for each degree of Fahrenheit to be $\frac{.0004538}{94.05} = .000004825$ inch, and for one foot this expansion would be .0000579 of an inch. By proceeding in the same way with the experiments on the other stones, we obtain the following results:—

Mean whole length in inches.	Common difference in inches for the whole length of stone for 1° of Fahr.	Common difference in inches for one inch for each degree of Fahr.
Granite . 94.05	.0004538	.000004825
Marble . 93.44	.0005297	.000005668
Sandstone 94.05	.0008965	.000009532
White Pine . .		.00000255
Hammered Copper		.00000944

To apply these results to the case in question, let us suppose two coping-stones, of five running feet each, to be laid in midsummer, when they have a temperature of 96° Fahrenheit; in winter their temperature may safely be assumed at zero, so that the total variation of temperature will be 96°; and if we suppose these stones to contract toward their centres, which would be the most favourable supposition as regards the tightness of the joints where a number of these stones are used, the whole length of stone put in motion by a change of temperature would be five feet. If the coping be of granite, the distance by which the ends of the stones would be separated, in consequence of one degree's variation, would be sixty inches, multiplied into $.000004825 = .0002895$; and for a variation of 96 degrees, this distance becomes $.0002895 \times 96 = .027792$ inch, giving a crack a little wider than the thickness of common pasteboard. For marble this crack would have a width of .03264, nearly twice the thickness of common pasteboard; and for sandstone .054914, nearly three times the thickness of pasteboard. These cracks are not only distinctly visible, but they allow water to pass freely into the heart of the wall. The mischief does not stop here: by this constant motion back and forth in the coping, the cement, of whatever kind the joints might be made, would be crushed to powder, and in a short time be totally washed by the rains from its place, leaving the whole joint open.

Observations on Saline Crystallization. By HENRY OGDEN, M.D., Extraordinary Member of the Royal Medical Society of Edinburgh. Communicated by the Author.

IN crystallizing small quantities of saline substances, it has frequently occurred to me to observe, that concentrated warm solutions may be cooled down to the ordinary temperature of the atmosphere, without depositing crystals. This property, as appertaining to sulphate of soda, is noticed in most of the systematic works on chemistry; but it is usually stated that it is necessary to defend the solution from the pressure of the atmosphere, by removing the vessel containing it from the fire, whilst in a state of ebullition, and immediately corking it up.

Gay-Lussac[*], however, has shewn, that atmospheric pressure does not necessarily induce crystallization; he found that if the liquid was covered with a film of oil of turpentine, it might be cooled without crystallizing. And more lately, Professor Turner[†] has succeeded in the same experiment, without the use of oil, " by causing the air of the flask to communicate with the atmosphere by means of a moderately narrow tube.

I have made a number of experiments on this subject, with many different salts; but I have never observed any relation to exist between the pressure of the atmosphere and the occurrence of crystallization in supersaturated solutions. The solutions have been cooled in vessels of all forms, from a narrow-necked flask, corked or not corked, to an open jar, or even a wide basin, without their crystallizing.

[*] Annales de Chimie, vol. lxxxvii.

[†] Elem. Chem.

In other cases, when the solution has been preserved from atmospheric pressure, the experiment has nevertheless failed, and the salt has crystallized. It requires some delicacy to conduct the experiment successfully, and mere protection from the atmosphere is no security against its failing. And when it succeeds, and a supersaturated solution is thus obtained in a well-closed flask or phial, I have found it as easily excited to crystallize by agitation without removing the cork, as when it is contained in an open vessel.

Atmospheric pressure, then, I do not consider as a condition at all facilitating crystallization in a supersaturated solution. Nor have I been able to determine what conditions are necessary. Crystallization often occurs without any obvious cause. The slightest agitation will sometimes produce it; even that occasioned by stepping across the room has appeared sufficient. Sometimes, again, the solution will bear to be briskly agitated several times daily, for many successive days, without crystallizing. A crystal of the same salt as that in solution will generally bring on crystallization, when placed in contact with the liquid; frequently, however, it fails to do so. As a general rule, the stronger the solution is, the more readily it is excited to crystallize by either of these means.

Sulphate of soda is the salt most frequently quoted as possessing the property of resisting crystallization; but it is by no means confined to it, or to any genus of salts in particular, alkaline, earthy, or metallic. Yet there are many salts of which I have never been able to form supersaturated solutions; if there was the slightest excess over the quantity soluble in cold water, it was invariably deposited on cooling, however carefully the experiment was conducted, and whether or not the solution was protected from atmospheric pressure. To this peculiarity I shall subsequently revert.

In stating that crystallization is quite independent of atmospheric pressure, I do not advance it as a new discovery, but rather as a point requiring additional investigation. The earliest mention I find of it, is in Gay-Lussac's paper published in 813; but he seems to have considered some protection necesr, and employed for this purpose a stratum of oil of turpen. Gay-Lussac also formed supersaturated solutions; with three or four different salts, shewing that the property was not peculiar to the sulphate of soda.

In the Repertory of Arts for 1818, vol. xxxiii, is a paper by Dr Ure, in which are detailed some curious experiments, tending to shew that negative electricity has a powerful effect in promoting crystallization. He operated on sulphate of soda, and also arrives at the conclusion that " neither the chemical properties of the atmosphere, nor its pressure, have any influence on crystallization."

Since then, Mr Graham of Glasgow has rendered it probable that the chemical properties of the atmosphere, or at least of some gases, are capable of inducing crystallization. His experiments consisted in throwing up small quantities of gases through mercury into supersaturated solutions of sulphate of soda, and he concludes that " air determines the crystallization of supersaturated saline solutions, by dissolving in the water, and thereby giving a shock to the feeble power by which the excess of salt is held in solution." Ammoniacal gas was found most efficient. The experiments are detailed in the Transactions of the Royal Society of Edinburgh for 1828. They also prove crystallization to be independent of the pressure of the atmosphere.

Notwithstanding these authorities, the doctrine of atmospheric pressure occasioning crystallization is yet very generally received, and may be traced in some of our best works on chemistry. There is a certain appearance of analogy between the conversion of elastic gas into a liquid by the force of strong pressure, and the conversion of a supersaturated liquid into a crystalline solid by the same means, which has greatly tended to give currency and plausibility to the latter doctrine. But the analogy is deficient in one very important particular. The liquefaction of a gas is attended with an immense reduction of volume; whilst in the solidification of a liquid, the volume undergoes little or no change. There are also circumstances in the mode of experimenting which may have been instrumental in perpetuating the error. When the cork or stopper is removed from a closed vessel containing a supersaturated solution, it is very liable to occasion a few almost imperceptible fragments of salt to be detached from the neck, which, falling into the liquid below, immediately excite crystallization; and this may readily be supposed to result from the entrance of the air. But supposing that there can be no suspicion of such an inaccuracy on the part of the operator, the agitation produced by the entrance of the air may, of itself, be a sufficient cause. The violence of the atmospheric vibrations caused by the discharge of a pistol is sufficient to break ordinary window-glass. Every one knows the loud report occasioned by puncturing the concave bladder forming the upper surface of a vessel exhausted by the air-pump. Of the same character is the sound in the common operation of opening a bottle of wine. The effervescence of champagne by a smart blow on the top of the glass with the palm of the hand, is a familiar illustration of a change in the constitution of a liquid, occasioned by a violent shock. In like manner, the concussion produced on the surface of a supersaturated liquid, by the sudden admission of air, may be amply sufficient to induce crystallization.

Besides the circumstance of pressure not being essential to crystallization, nor certain to produce it, there are other minor points in the nature of supersaturated solutions, into which I was led to inquire, and which it may not be deemed irrelevant to mention. These, as well as the first, will be sufficiently explained by the following desultory experiments. It is necessary to premise, that, in all cases, the solutions were unprotected from atmospheric pressure, except where the contrary is expressly stated.

1. A hot solution of carbonate of soda nearly saturated, and contained in a six-ounce phial, was allowed to stand in a hot sand-bath until the whole was cool. Twenty-four hours afterwards it was perfectly fluid, but being gently moved, it solidified.

2. A strong solution of sulphate of soda was filtered whilst hot into two phials, one of which was corked immediately, and the other was left open. At the end of three hours, the contents of each remained fluid, and each became solid on being agitated.

3. A strong solution of sulphate of soda was filtered whilst hot into an open jar three inches in diameter. Twenty-four hours afterwards, it remained perfectly fluid. A crystal of the same salt being dropped into it, the whole solidified in a few seconds.

These experiments clearly demonstrate that atmospheric pressure alone does not produce crystallization.

Owing to its remarkable property of being most soluble at the temperature of 106° Fahrenheit, sulphate of soda is particularly well adapted for these illustrations. It does not require so gradual and careful a reduction of temperature as other salts; the solution may be filtered at any high temperature near the boiling point, for it has no tendency to crystallize until it has fallen to 106°. With many salts, crystallization commences with the least reduction of temperature, and if the attempt is made to filter their solutions fully saturated, they immediately begin to crystallize. It is therefore necessary to use them somewhat under saturation at the boiling heat.

4. A strong solution of muriate of lime was closed up whilst hot, and allowed to cool. It was several times freely agitated, without producing any effect. On exposing the phial containing it to a blast of cold wind, it crystallized. The same effect may be produced by dashing cold water on the outside of a vessel containing a supersaturated solution.

The seclusion from atmospheric pressure in this case had no effect in preventing crystallization.

5. To ascertain if a supersaturated solution was capable of exhibiting its proper chemical qualities without crystallizing, I again heated and gradually cooled the vessel containing muriate of lime. Into the cold supersaturated solution thus formed, I let fall two drops of concentrated sulphuric acid, which produced a solid crust of sulphate of lime, and evolved fumes of muriatic acid, but did not induce crystallization.

6. The last experiment was repeated, shaking the mixture at the time the sulphuric acid was added. It did not, however, crystallize.

7. Decomposition was likewise effected by adding oxalate of ammonia to the same solution. No crystallization was produced.

8. Nitrate of barytes was dropped into a supersaturated solution of sulphate of magnesia, forming sulphate of barytes and nitrate of magnesia. The solution did not crystallize.

The only result of the four last experiments is, to shew that the salts in supersaturated solutions continue to exert chemical elective attraction, without necessarily reverting to the state of ordinary saturation. This might have been considered doubtful, in consequence of the feeble affinity by which the salt is held in solution. A drop of alcohol, by combining with the water, separates a nucleus, and thus crystallizes the whole.

It occurred to me that the peculiar constitution of supersaturated solutions, on which depends their power of resisting crystallization, might be completely changed by the admixture of a portion of mother-water of the same salt, which had afforded crystals;—as the fermentative process is established by means of a ferment, in a liquid capable of undergoing fermentation.

9. With this view I again prepared the supersaturated solution of muriate of lime, and poured into it a drachm of mother-water of the same salt. The vessel was briskly agitated, but no crystallization succeeded.

Hence, whatever may be the molecular state or constitution of a supersaturated solution, it is quite compatible in mixture, or in combination, with a solution simply saturated.

In some supersaturated solutions crystallization proceeds slowly; in others, especially if concentrated, it proceeds so rapidly when a nucleus is obtained, as to give the idea that the disposition to crystallize is communicated instantaneously, like an electric shock, to the whole mass. The following experiment, however, proves that this is not the case.

10. An evaporating dish was prepared with a supersaturated solution of acetate of soda, and left for twenty-four hours in a state of perfect quiescence. A crystal was then carefully dropped in at one side. When about one-fourth of the mass had crystallized, a portion of the remaining liquid was poured into another vessel, where it continued fluid until the next day; it also was then crystallized by the same means.

11. A vessel containing supersulphate of alumina and potash, which had already been the subject of experiment, was heated in a sand-bath until all the salt was redissolved. On cooling, about one-third of it crystallized; the rest remained fluid until it was shaken.

This experiment shews that the presence of crystals in a supersaturated solution does not necessarily determine crystallization.

12. There does not appear any limitation to the time which a supersaturated solution may retain its fluidity. Phosphate of soda was kept in that state for the space of eleven days, then shaken for several minutes, and poured into another vessel. It continued perfectly fluid until a crystal was dropped into it.

I have already stated, that with some salts I have never succeeded in forming supersaturated solutions, the excess being always deposited on cooling. The mode of determining this property may be varied in many ways; that which I generally adopted, and found most convenient, was the following.

Any given quantity of a salt is dissolved in a quantity of distilled water less than is requisite to form a saturated solution when cold, but greater than is requisite to form a hot saturated solution. The operation is performed in a glass-flask over a charcoal-fire, and after being kept a few seconds at a boiling heat, the solution is filtered through paper into another clean flask, in which it is again brought to a boiling heat. The fire is then covered with an iron-plate, by which it is slowly and gradually extinguished. The vessel containing the solution is suspended over the iron-plate, and within an inch of it; every thing is then left undisturbed for ten or twelve hours. When cold, if the salt is found to have crystallized, it is owing to one of two causes; either it is a failure of the experiment, the salt being capable of forming a supersaturated solution; or the salt is one of those which are incapable of existing in that state. To remove the doubt, the vessel and its contents must be again heated and cooled; this process must be frequently repeated; and if there is uniformly a deposition of the superabundant salt, it may be concluded that it is incapable of forming a supersaturated solution.

The first of the following tables exhibits those salts with which I have formed supersaturated solutions; the second, those which appear incapable of that state:—

TABLE I.

Carbonate of soda.
Sulphate of soda.
Tartrate of soda and potash.
Biborate of soda.
Acetate of soda.
Phosphate of soda.
Ferrocyanate of potash.
Supersulphate of alumina and potash.
Bisulphate of potash.
Tartrate of potash and antimony.
Nitrate of ammonia.
Oxalate of ammonia.
Sulphate of magnesia and ammonia.
Sulphate of magnesia.
Muriate of lime.
Muriate of barytes.
Acetate of lead.
Sulphate of iron.
Sulphate of zinc.
Sulphate of copper.
Bichromate of potash.

TABLE II.

Muriate of potash.
Nitrate of potash.
Sulphate of potash.
Chromate of potash.
Chlorate of potash.
Nitrate of lead.
Bichloride of mercury.
Nitrate of barytes.
Muriate of ammonia.
Sulphate of ammonia.
Sulphate of magnesia and potash.

In the progress of these experiments I could not avoid observing, that the principal characteristic difference between these two sets of salts was, that the first contain water of crystallization, whereas the second are anhydrous. So generally true is this, that it was with some surprise that I found bichromate of potash, an anhydrous salt, to form an exception. Of all the rest of the salts in the first table, the crystals are hydrates. In the second table there are two exceptions, sulphate of ammonia, and sulphate of magnesia and potash, both of which are crystalline hydrates. These three exceptions (and many others may yet be found) annul the distinction between hydrated and anhydrous salts, and invalidate any general rule which might be founded on it.

Whilst operating on sulphate of soda, I frequently observed a deposition of a portion of the salt, in the form of brilliant transparent crystals, which became white and opaque when crystallization was induced in the remaining liquid. This phenomenon is described by Mr Faraday in the Journal of Science, vol. xix.; he finds the crystals to contain eight only, instead of ten, atoms of water of crystallization. Mr Faraday refers to their production in closed vessels only. I have generally experimented with open vessels, and all the instances I have seen have been in such. In one case the vessel was an open dish four inches wide.

But there is another phenomenon in the habitudes of sulphate of soda with water, which, so far as I know, has not been noticed by any writer. Under certain circumstances, a cold supersaturated solution has the power to dissolve an additional quantity of the crystallized salt. Not only does it dissolve it, but the solution is greatly facilitated by agitation, unless, by some capricious incident, the agitation should excite it to crystallize. To illustrate this point, four ounces of sulphate of soda may be dissolved in four ounces and a half of hot distilled water in a glass-flask. The superfluous salt must be allowed to crystallize, and the vessel containing both salt and mother-water is to be placed in a dish containing sand, and exposed to a temperature of 120° or 130° F. in a common kitchen-oven. When all the salt, with the exception of about a drachm, is dissolved, the flask is to be removed and carefully cooled. If this is successfully done, it is not accompanied by any deposition of crystals. In this stage, the flask contains a cold supersaturated solution, along with the portion of salt which remained undissolved by the heat of the sand-bath *. It is now to be gently inclined to one side, so as to elevate the undissolved crystals into the superior part of the liquid. After standing an hour or two in this position, the most elevated part of the salt will have been dissolved; and, the vessel being inclined in another direction, another part of the salt is in its turn raised to the superior part of the liquid, and there dissolved.

In repeating the experiment, I have generally shaken the vessel briskly, and found the crystals to dissolve with much greater rapidity in consequence; though this very agitation has sometimes induced crystallization before the solution had been completed.

Thus, a solution of sulphate of soda more than saturated, and which has stood two, three, or four days in a cool room, actually continues to exert a solvent power on salt of its own kind.

It was suggested to me, that the undissolved salt might not be sulphate of soda, but some accidental impurity, soluble in

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* Furnishing another proof that the presence of crystals in a supersaturated solution does not necessarily determine crystallization.

virtue of the known power possessed by saturated solutions to dissolve a little of another different salt. I had no reason to doubt the purity of the salt which I employed, and I tried many different specimens, and always obtained the same result. To put it still further to the test, I prepared the solution as above described, and when cool, poured the supersaturated liquid into another vessel, and crystallized it. I then returned the mother water into the flask containing the undissolved salt; but it had no solvent effect on it. So that the very same salt which would have dissolved in a cold supersaturated solution, was insoluble in a solution simply saturated, which is the condition of mother water after the deposition of crystals.

Doubtless this power of solution possessed by the supersaturated liquid, has its limits; as the first stage, or simple saturation, is the limit of the solvent power of cold water; but what is the limit of supersaturation, yet remains to be ascertained. And the inquiry is beset with some difficulties; for, independently of the great liability of strong solutions entirely to crystallize, the process is sometimes interrupted by the deposition of the brilliant quadrangular crystals, containing eight proportionals of water of crystallization. Sulphate of soda may indeed be considered capable of three stages of saturation; the first is the limit of the solvent power of cold water; the second is the liquid which has deposited quadrangular prisms; and the third contains a still greater quantity of salt. The next experiment illustrates these three stages.

13. A supersaturated solution of sulphate of soda, with a portion of undissolved salt remaining at the bottom of the vessel, was allowed to stand at rest for four days. It was then briskly agitated, and most of the salt was dissolved. All this time it had existed in the third stage of saturation, although not at the limits of that stage. The next day there had been a considerable deposition of brilliant and transparent crystals. The remaining liquid was now in the second stage of saturation. The whole was again repeatedly shaken in the course of three hours, without effecting any change. Two hours afterwards, the liquid suddenly became nearly solid, without any apparent cause; the small quantity of fluid now remaining was in the first stage of saturation.

I never could calculate with any certainty on the phenomena which these experiments would present. They were often total failures, and it was only by frequent repetition of them that their peculiarities could be observed. The following is an instance of the irregularities I met with.

14. A pound of crystallized acetate of soda was fused in its water of crystallization, and poured into a clean glass retort. Six hours afterwards, it was cold and perfectly fluid, with the exception of a mass of crystals about the size of a hazel nut, which lay at the bottom of the retort, and a few smaller masses which were floating in the liquid. It was several times shaken without its shewing any tendency to crystallize. A small crystal of the same salt being then dropped in, the entire mass became solid in a few seconds. The evolution of sensible heat which attends the transition from the liquid to the solid state, was in this case very considerable.

15. Sulphuric acid sufficient to decompose the acetate of soda was then added, and the acetic acid was drawn off by distillation. Water was poured on the residuary sulphate of soda when cold. The next day about two-thirds of the salt had dissolved; but on attempting to pour it out of the retort, it suddenly crystallized, and became a semifluid mass.

SUNDERLAND.

On the Magnitude of the Ultimate Particles of Bodies; Infusory Animals not formed immediately from Dead Matter; Extraordinary Minuteness of the Infusoria; Improved Arrangement of the Infusoria; Marvellous Multiplication of the Infusoria; Estimate of the relative value of the Microscopes of Chevalier, Ploessel, and Schiek. By Prof. C. G. EHRENBERG of Berlin.

Magnitude of Ultimate Particles.—Within these few years, the atomists have become pretty confident in their doctrines relating to the minute particles of bodies. They have not rested satisfied with viewing atoms as ideal unities of an infinite degree of minuteness, but have sought for approximative numerical expressions of their magnitudes. Nay, many bold theoretical experimenters of the present day seem to have fallen little short, in their own imagination, of in reality seizing the ultimate elements of bodies, and of reconstructing them at pleasure.

Newton long ago taught us to believe that the elements of colour were of tolerable magnitude. His words are, " Could the power of the microscope be so increased as to represent objects at a foot distance, magnified 500 or 600 times above what they appear to the naked eye, I imagine that we might discover some of the coarser elements which enter into the formation of colours; and that, with a microscope which magnified 3000 or 4000 times, we might recognise them all, even those which form the black colour." Supposing that Newton has correctly estimated the natural power of human vision, his elementary particles, as appears from the following observations, would, for the red colour, not exceed a minuteness of $\frac{1}{50000}'''$ in diameter, between which and $\frac{1}{144000}'''$, all coloured particles, not excepting the black, would be included*. But it is probable that Newton's estimate of the acuteness of vision is below the truth, when these elementary particles would become considerably larger. But, as has been already observed by Herschel, in his Optics, it must not be forgotten that Newton made a marked distinction between the elements of colours and atoms, as well as later philosophers, although he does not expressly say so. In the above passage, Newton does not speak of atoms, but of colouring particles.—(*Traité d'Optique,* 1704; lib. ii, part iii; Ed. Franc. 1720, p. 357.)

The small magnitudes which have been required for the explication of the phenomena of light upon the theory of undulation, are exactly enough determined by calculation, but they can only be considered as hypothetical, not as really observable quantities, as the whole theory, however great its probability, still requires more complete demonstration. The smallest length of a wave of light on this theory, calculated from the most exact data, does not exceed $\frac{1}{100000}''$, or about $\frac{1}{8000}'''$. But, as the particles of ether must be considerably smaller than their undulatory motions, this number gives a tangible limit for their maximum, although somewhat arbitrary, and therefore one expression for their degree of minuteness. If we were to conclude the minuteness of these elementary particles from the want of weight of highly condensed light, or masses of ether, the limits of these maxima must be considerably extended. All these, however, are mere hypothetical magnitudes when we come to express them in numbers.

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* The general reader may be informed, that 0″ is the mark for inch, 0‴ for line or 12th part of an inch.

The coloured phenomena between glasses which are nearly in contact, permit likewise of some inferences regarding the magnitudes of the so-called elements of colour. Newton has already fixed at $\frac{1}{178000}''$, the smallest interval, which gives a white colour, which is a little more than $\frac{1}{14000}'''$, and Haüy has calculated, from the different refractive powers of mica, that a plate of this substance, which produces the same effect as the above layer of air, must be $\frac{1}{400000}$ of a millimeter, or $\frac{1}{900000}'''$ in thickness.

With respect to solid inorganic as well as organic bodies, Robert Brown's microscopic measurements of the years 1827 and 1829, fixed the size of the smallest observable particles which he saw in active movement, and of a globular form, at about $\frac{1}{20000}''$ to $\frac{1}{30000}''$, or from $\frac{1}{2000}'''$ to $\frac{1}{2300}'''$ in diameter.

J. F. W. Herschel, in his Optics (1829), says, that he has seen bodies, through an Amician microscope, magnifying 3000 times, but that he was far from supposing that he had at all succeeded in resolving bodies into their constituent atoms.

Infusory Animals not formed directly from dead matter.—M. Dumas, the chemist, from his own observations, maintained, in 1825, that the elementary globules of dead organic matter might be seen and counted by the aid of a good microscope; that they formed other larger bodies by the simple union and augmentation of their masses, which were at first infusory animalcules, capable of being resolved by the electric shock into their primitive elements, when they assumed the form of a raspberry.—(*Dict. Class. d'Hist. Natur. Article* GENERATION, p. 195.) In the same place, p. 81, the author believes that, in the present state of chemistry, it was possible to form synthetically an artificially organized matter, and concludes, "If infusory animals could be obtained in this way, the Bonnetian theory of generation would be overturned."

Koelle also gives a peculiar chemical microscopic explanation, (*Kastner's Archiv. f. Naturlehre*, xii. 348, 1827.) He says: Zymom consists of microscopic globules, and forms with Glyadine Gelatin (p. 350.) Zymom is the substance from which the simplest organic forms are produced on the occurrence of favourable circumstances (p. 352.) The globules of milk and blood are zymom; gelatin, caseum, starch, sugar, &c. contain zymom, (p. 350.) Silica assumes first a vegetable structure, and from the zymom formed from thence arises animal life, (p. 358.) Vegetable matter can be immediately changed into an infusory animal (p. 360.) In favourable circumstances, the different kinds of infusory animals can be formed from zymom (p. 358.) The first infusory animal, the lowest organic form, is an animated zymom globule (p. 358.) Zymom may, in certain points of view, be considered as an egg, (p. 360.) The yolk of egg consists of zymom united with mucus, (p. 357.) These are not hypotheses but facts, (p. 361.)

That the origin of many organisms is the effect of putrefaction or fermentation, and therefore a purely chemical process, is a very old opinion, and therefore could not fail of being reproduced in an improved form in modern times. Gruithuisen (*Gehlen's Journal der Physik*, viii. 519. 1809) has characterised the formation of the smallest organic forms as a peculiar kind of fermentation, and specifies, besides the *vinous* and *acetous*, the *infusorial* fermentation as a source of organization. Formerly the autochthoniens were supposed to have been formed in this way, afterwards it was limited to insects, and the plants produced during the process, but now insects and the larger plants have been withdrawn from this category. It has since been extended a little to include the Infusoria and Fungi, on account of their difficulty of observation, but from which, as our observations have shewn, it must be equally withdrawn.

Berzelius, who notices the subject in his classical *Lehrbuch der Chemie*, but does not give any observations of his own, considers as probable the fact mentioned by others, that dead organic matter, on being moistened with water, generates infusory animals; and views Professor Hornschuch's idea as not improbable, that the *prima germina rerum*, or infusoria, may be formed in different bodies, under certain external influences. In other respects he has followed MM. Dumas and Milne Edwards in the doctrine of organic atoms; and the inorganic atoms, which have been of so much service to the doctrine of chemical proportions, by the simple process of addition and subtraction, are more or less ideal unities, which will long continue of the most decisive practical benefit to chemistry.

Quite recently Muncke of Heidelberg has observed the march of organic bodies in infusions with a microscope of Plossel, and has arrived at the result, that a passage of organized matter takes place from vegetable to animal life, and from animal to vegetable life.—(*Isis*, 1831, p. 1083.)

I must state, in the first place, that the whole range of my microscopic observations are completely opposed to the prevailing opinion, that infusory animals or fungi can be produced by simply pouring water upon dead organic substances. It must be admitted that such appearances are deceiving; but when examined minutely, sometimes one kind of infusory animals, sometimes another, appear under the most similar modes of treatment, and I have never been able to obtain certain forms by means of certain infusions, although this is stated in all the manuals, and has succeeded with all the earlier observers. According to my results there are certain forms, but few in number, and these the most diffused, whose ova or individuals are found in every kind of fluid, even in those of what are reckoned the most poisonous parts of plants. Blainville (*Dict. des Soc. Nat. Art. Zoophytes*) has also arrived at the same opinion with regard to the *generatio equivoca* from his own observations. I have often laboured in vain to produce, at will, a certain species of organic bodies in little glass tubes, although in others containing the same water, and situated in the same circumstances, they multiplied with the utmost profusion.

Extraordinary Minuteness of the Infusoria.—My observations, in regard to the smallest organic parts, have enabled me to ascertain the following smallest magnitude, as actually existing and discernible by the senses:

By means of the microscope, I saw distinctly monades in which the greatest diameter of the body was from $\frac{1}{1500}'''$ to $\frac{1}{2000}'''$ of a line. This, which is the smallest of known animals, I have named *Monas termo,* is the same as that described by Otto F. Muller, under this name. In the largest individuals of this animal, I was able, by colouring the liquid, to discover in some, the larger, six, in the smaller, four sacs or stomachs; and in some of them the stomachs did not occupy the half of the whole animal. Such a stomach of the Monas termo, therefore, if the animal is only $\frac{1}{1500}$ part of a line in size (if there are only four stomachs which occupy the half of the animal), is $\frac{1}{12000}$ part of a line in magnitude, consequently five times smaller than the smallest molecules observed by R. Brown. We observe in the forepart of these animals, as in all the monades, a violent projection of still smaller bodies than itself, as soon as these come near to it, hence these have probably a wreath of ten or twenty feelers around the anterior mouth opening, as in the Monas pulvisculus, and the other still larger monades. It is probable that each of the stomachs which are filled in our experiments with colouring matter, contain more than one atom; if each stomach contains three coloured atoms, this affords a proof of the existence of red and dark blue particles of colouring matter floating in water, with a magnitude of $\frac{1}{36000}$ part of a line, $\frac{1}{432000}$ part of an inch in diameter; and if the same objects are calculated according to the smallest animals we have observed, which are $\frac{1}{2000}$ of a line in magnitude, and sometimes contain four coloured points in the hinder part of the body, these latter parts, which are no longer individually distinguishable, even by a power of 800, but are distinguished in the aggregate, have a magnitude of $\frac{1}{48000}$ of a line, or $\frac{1}{378000}$ of an inch, which is twenty times smaller than the smallest molecules of R. Brown. We may also notice the fineness of other parts of these *living organic beings.* The small stomachs of the monas appear isolated in the body, and sharply bounded. In larger infusoria which are $\frac{1}{48}'''$, or upwards, in diameter, we see these receptacles as distinct bladders, and there is no reason for assuming another structure in the coloured cavities observed in the smallest of the monades in our experiments. If we assume the thickness of the walls of the stomach as $\frac{1}{10}$ of its diameter, it amounts in the Monas termo, having a diameter of $\frac{1}{2000}'''$, where the stomach appears as the $\frac{1}{8}$th part of the measurable length of the whole animal, consequently $\frac{1}{16000}'''$ in diameter, to $\frac{1}{160000}$ of a line or $\frac{1}{1920000}$ of an inch, and as there is reason for supposing that the walls of the stomach contain vessels, it affords a still further minuteness of atoms. But magnitudes even smaller than these may be pointed out. In the Polygastric infusoria there is an ovarium. The grains of this ovarium are as 40 to 1 in the female of the *Kolpoda cucullus*, in others as 80 to 1; and they appear to increase in fineness as the body diminishes in magnitude. Is it not probable that it is only the transparency and the imperfection of our microscopes that prevents us from observing a similar ovarium in the monades, which are similarly organized, so that it cannot be overlooked that there may be young monades contained in the ovum, or which have escaped from it, in which the diameter of the whole body measures only $\frac{1}{60000}'''$ to $\frac{1}{80000}'''$, and which also are provided with stomachs, which, according to the same relation, will have a diameter of from $\frac{1}{480000}'''$ to $\frac{1}{640000}'''$? The walls of these monadal stomachs will be about $\frac{1}{4800000}'''$ to $\frac{1}{6400000}'''$ in diameter.

By the kindness of Professor Ensler of Berlin, I have been enabled to make many observations with a solar microscope. On viewing the *Monas atomus*, strongly filled with indigo, I discovered in the intervals the shades of smaller monades, which could not have amounted nearly to $\frac{1}{2000}'''$, but which were quite invisible in the water when examined with Chevalier's microscope, perhaps on account of their transparency. Whether these bodies were the young of the *Monas atomus*, or independent species, it follows that $\frac{1}{2000}'''$ is not the limit to a size of organic forms which can be distinctly seen.

Improved Arrangement of the Class Infusoria.—Hitherto I had only been able to observe in *Infusoria Polygastrica*, the muscular, digestive, and generative systems. In one genus only of the class could I detect the points of the eyes. Recently I have been (viz. the *Englena*) able to detect the eye-points more frequently, so that now the genera possessed with eyes can be enumerated, comprehending sixteen species. Among these forms there are some eye-bearing monades $\frac{1}{192}'''$ in diameter. Thus the traces of a nervous system descend to the monades.

I have extended farther my first endeavour to class the Infusoria according to their internal organization. The digestive system furnishes, in each of the two classes, only four differences. The Polygastrica fall under the Anentera, those without intestine; the Cyclocæla, those with a circular intestine; Orthocæla, those with a straight intestine; and Campylocæla, those with a curved intestine. The Rotatoria fall under the Trachelogastrica, long-throated without stomach; Cælogastrica, long intestine without a stomach, and with a short throat; Gasterodela, those with a stomach; Trachelocystica, those with bladders. The intestine of the latter is very peculiar.

The Rotatoria alone, from my further observations, have been capable of being divided according to their dentary organs. They fall first under three groups: Agomphia, toothless, they are few in number; Gymnogomphia, free toothed, they are the most numerous; Desmogomphia, hook-shaped teeth. Those with free teeth fall under two great natural divisions, viz. the Monogomphia with one tooth in each jaw, and the Polygomphia, with many teeth. The Desmogomphia, whose teeth are not free but inserted into a cartilaginous plate, fall also into two natural subdivisions, the Zygogomphia with teeth disposed in pairs, and Lochogomphia, with teeth in rows, so that the following scheme may be formed:

AGOMPHIA.	GYMNOGOMPHIA.		DESMOGOMPHIA.	
	Monogomphia.	Polygomphia.	Zygogomphia.	Lochogomphia.
I.	II.	III.	IV.	V.

Marvellous Multiplication of the Infusoria.—I have also made some observations on the development and multiplication of the Infusoria, which I deem among the most important of all my researches. I have observed for eighteen days successively a single *Hydatina senta*, and as it was perfectly grown when I singled it out, and did not die of old age, being accidentally destroyed, the life of this animal must be more than twenty days. Such an individual, when circumstances are favourable, is capable of a fourfold propagation every twenty-four—thirty hours. It can in this time bring forwards four eggs from the embryo state to the exclusion of the young. But this fourfold increase in the space of a day, when no obstacle intervenes, and the same individual gives, in ten days, forty eggs, and raised to the tenth power (therefore on the 10th day) a million of individuals from *one* mother; and on the 11th day four, on the 12th sixteen, millions, &c. Although this productive power is the greatest which has been yet observed in nature, far exceeding that of insects, it is far from attaining that of the Polygastrica. In the *Paramæcium aurelia*, which is $\frac{1}{12}'''$ in size, and which has been ascertained to live several days, a doubling of each individual by transverse division has been observed within twenty-four hours, its rate of increase is therefore double that of the preceding. But as these animals, besides division, also propagate by eggs, and these eggs are not separated from the parent singly but in masses, and as they also form gems, the possible increase within forty-eight hours becomes quite innumerable. Who can wonder that, under such circumstances, fluids should, with the brood of two or three days only, swarm with these animalcules?

Estimate of the relative Value of the Microscopes of Chevalier, Ploessel, and Schiek.—The following are my individual views regarding the microscopes of Chevalier, Ploessel, and Schiek, which are the best of the present day. The advantages of all these instruments rest upon the discovery of Selligue, and the most important advances upon this basis have been certainly made by Chevalier.

The chief practical advantages of the microscope of Chevalier are,—extent of field of vision, distinctness of outline, even with the highest powers, high magnifying power, a sufficient distance of the object-lens from the object, which amounts to a line even with the highest powers, simplicity of apparatus, and, consequently, a very moderate price.

Ploessel's microscope, compared with that of Chevalier, is distinguished by the following properties. With as extensive a field of vision, it has still greater, perhaps too great, light, it magnifies nearly double the diameter with great distinctness of outline; its use is more convenient under high powers, from the employment of several eye-glasses, instead of the elongation of the tube required by the former. The inconveniences of Ploessel's microscope are,—its almost entire uselessness for the observation of fluids, on account of the small distance of the object-lens from the object under the high powers, and the impossibility of employing apparatus for pressure, not even mica. It is also, on account of its varied adjustments, and particularly a very fine screw-micrometer, twice the price of Chevalier's; therefore only half as useful considered in the abstract. Its form is much handsomer than that of Chevalier, and more convenient for use.

Schiek's microscope unites the advantages of both of the others. Its field of vision is extensive, extremely sharp and clear illumination, even to the highest powers, appearing to me much clearer than either of the others when I compared them together. Its magnifying powers equal the highest of Ploessel's, therefore twice that of Chevalier's, accompanied with considerable focal distance, permitting of the use of water and pressure for the most minute objects. Its size is smaller, and form more convenient and handsomer than the others without being weaker. From the simplicity of its arrangements, the price is very moderate, and therefore particularly adapted for the working naturalist.

The greater clearness of the image, and sufficiency of light, is a step made by M. Schiek which is a clear gain to optics; which, united to all the other advantages, have appeared to me quite new and surprising.

Outline of the Geology of the Bhurtpoor District. By JAMES HARDIE, Esq. Bengal Medical Establishment. Communicated by the Author.

THIS district may be described as forming a portion of the south-western boundary of the Valley of the Ganges and Jumna. It presents a level platform, elevated about sixty feet above the bed of the latter river, and is in most situations covered with the usual alluvium of the Gangetic provinces. The country is fertile and highly cultivated, and impresses us with a favourable idea of the industry and enterprise of the Jauts, as contrasted with their Rajpoot neighbours of Jeypoor. The lesson taught them by the fall of their far-famed capital in 1825, has not been thrown away upon the inhabitants; and in no part of India does the English traveller meet with more civility and attention than in this district, where a few years ago he was treated with insult and contempt.

The rocks which immediately underlie the Gangetic deposits in some few situations, appear near the surface, and are quarried for architectural purposes; while strata of an anterior date to these here and there crop out, forming, especially in the northern portion, small detached hills, which are generally topped by a village or stronghold. To the west the Bhurtpoor district is flanked by a belt of rocks of the secondary class, which stretches in a north-easterly direction from the ancient city of Biana, situated on the south-western portion of the district. This belt is interposed between the newer strata just alluded to, and the decidedly primary formations of the Jeypoor and Ajmeer territories: its eastern limit is marked by a low hill range, seen a short distance to the westward of the city of Bhurtpoor.

The sandstone quarries which have for centuries supplied all this portion of India with materials for building, are situated in the Bhurtpoor district, and, as these are important, both in a statistical and geological point of view, I shall, in the first place, communicate what little information I have been enabled to collect relative to their natural history.

Of the sandstones there are *three* varieties. No. 1. is a close-grained argillaceous sandstone, more or less slaty; of a uniform dark red colour, so soft as to be scratched by the knife, and apparently composed of small particles of quartz, cemented together by a ferrugino-argillaceous basis: minute scales of mica are distributed through the mass, to which circumstance it apparently owes its slaty texture. No. 2. is also a close-grained argillaceous sandstone. This is a very beautiful variety, its colour is dark red, speckled with white spots, which are generally roundish, and vary from an inch in diameter to the size of a pin's head. This rock is less schistose than No. 1, contains less mica, and, when slabs of it are properly cut and squared, has, at a little distance, exactly the appearance of a fine red porphyry. No. 3. is a rock similar in point of texture and composition to the last, but is of a uniform salmon-colour, passing into greyish-white. It can with difficulty be obtained in slabs less than four inches thick, while No. 1. may be had in plates less than one-half an inch.

The above rocks are all of them employed in architecture, and are remarkably free working stones. Slabs of the slaty variety, spanning from beam to beam in flat roofs, have been substituted by my friend Lieut. J. T. Boileau of the Bengal Engineers, instead of the bridging joists and tiles in common use; and he has found that the roofs completed upon this principle have, in every instance, proved perfectly water-tight, and that the terrace laid over the stones has invariably dried more uniformly, and freer from cracks, than when bridging joists have been used. By a series of experiments, Lieutenant Boileau has demonstrated that no apprehension need be entertained in regard to the transverse strength of the sandstones, objections on this head having been urged against his plan, while the advantages attending its adoption are invaluable, inasmuch as it secures the buildings against fire, and, what is of still more importance, renders them safe from the attacks of white ants. The annexed Table will shew the results of Lieutenant Boileau's experiments, which are interesting both in a mineralogical and economical point of view.

By a reference to the Table, it will been seen that the red-sandstone does not lose much, if any, of its strength by being saturated with water, which renders it particularly fit for the purposes of roofing; and though, in its dry state, it is far inferior to the salmon-coloured variety, (as 11 to 17 nearly), yet, when wet, it is something superior to it, the latter variety losing about one-half its strength by immersion in water. For the cause we may look to the difference observable in the arrangement of the component particles of either variety.

Table shewing the Results of Experiments made on Sandstone Slabs, from the Quarries near Agra, measuring 4½ feet in length, 12 *inches in breadth, and* 1½ *inches in thickness.*

No.	Description.	Specific gravity.	Weight of a cubic foot in lbs.	Weight of the specimen dry.	Weight of the specimen wet.	Weight in lbs. causing fracture.	Deflection in inches at the instant of fracture.	Value of e in the equation $W = \frac{B.D^2}{L} e$
1	Red sandstone slabs, dry,	2275	142	80	...	304	.362	45
2	... ...	2346	146	82⅓	...	390	.50	57
3	... ...	2346	146	82½	...	482	.75	71
	Means,	2322	144⅔	81⅔	...	392	.537	57
4	Salmon-coloured do. dry,	2389	149	84	...	555	.75	82
5	... ...	2361	147	83	...	617	.50	91
6	... ...	2260	141	79½	...	555	.50	82
	Means,	2336	145⅔	82⅙	...	575	.58	85
7	Red sandstone do. wet,	2268	141	79¾	83¾	344	.75	50
8	... ...	2190	136	77	82	498	.625	75
9	... ...	2190	136	77	81	316	.75	46
	Means,	2216	137⅔	78½	82¼	386	.708	56
10	Salmon-coloured, do.	2012	125	70¾	74¾	266	.75	39
11	... ...	2140	133	75¼	78¼	203	.100	30
12	... ...	2131	133	75	77¾	432	.625	64
	Means,	2094	130⅓	73⅔	77^{1}	300	.791	44

The majority of the buildings of the Agra and neighbouring districts are constructed of the sandstones under review; and among these are some of the most splendid edifices of Hindosstan. The fort of Agra, the gateway of the Taj, the tomb of Akber at Secundra, the magnificent gateway to the shrine of the Mussulman saint Salim Chisti at Futtypoor Sickra, and the far-famed Kutib Minar at Delhi, may be enumerated among the many edifices constructed of the red and speckled varieties; while the palace in the garden at Deeg, some magnificent modern temples at Muttra, and the tombs of Buldeosing and Surujmull at Goverdhan, the classic haunts of the Indian

Apollo, are examples of the salmon-coloured variety [*]. The roofs of many of these buildings are very wide, but nevertheless covered in by a series of slabs, spanning from wall to wall.

The dark red colour and brick-like aspect of the first variety, as contrasted with the pure white of the Makrana marble, of which the cupola pavilions and trellices of the buildings are generally formed, give to many of the most stupendous monuments of Mogul architecture, a fantastic and party-coloured appearance; and it is impossible not to regret that the lighter coloured varieties had not come into general use at an earlier period. To the employment of the red sandstone there is a still stronger objection. This rock seems but little capable of resisting the decomposing effects of the atmosphere; hence it is that so few of the edifices, for which Agra was once so famous, now remain entire, and of the few which still survive, the majority are rapidly falling to decay. The splendid mausoleum of Akber at Secundra is likely soon to meet the fate of its fellows; while at every step we take we perceive the most stupendous ruins of palaces, and courts, and gateways, which less than two centuries ago were the favourite haunts of the monarchs of Hindostan. In every climate this is a most serious objection, but in a climate like India it is quite insurmountable. In, comparatively speaking, modern days, an Indian monarch, surrounded by a court more gorgeous than Europe ever witnessed, pronounced this capital the "first city in the world [†]," destined to be eternal! We may smile at the vanity of Jehanghir; but let us not neglect the lesson which the fate of Akberabad [‡] has so emphatically taught us. A day must come, distant though that day may be, when we, like those who have preceded us, must resign to others the mighty empire of the East; and I know not if at this hour there exists in India a single edifice of British construction which will survive our fate, even for a few years. We cannot but regret that this should be the case: independent of every other consideration, we feel a sort of melancholy pleasure in the idea that we shall leave behind us some monument of our greatness, something amid the scenes of our achievements, to tell to future generations of the existence of one of the most extraordinary empires that the world ever witnessed, and to mark the spots where our footsteps have been. In this point of view, the labours of the geologist acquire additional importance: but to our subject.

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[*] For a description of these magnificent edifices see Bishop Heber's Travels.

[†] See the Life Jehanghir, written by himself.

[‡] The Mussulman name of Modern Agra.

The salmon-coloured sandstone is less liable than the red to be affected by atmospheric influences. The former is a remarkably fine freestone, and may be had in slabs of any magnitude; it is admirably adapted for paving and hearth stones; and is even capable of being used in the finer kinds of ornamental architecture. A remarkably handsome chimney-piece of this rock is in the possession of Colonel Lockhart, our political agent at Bhurtpoor, which, in chasteness of design and minuteness of sculpture, could scarcely have been surpassed had the finest marble been employed in its fabrication *.

The sandstones under review belong to that great formation so extensively distributed throughout Hindostan, and which has been pretty generally considered as the type of the new red sandstone of England. The rocks of this series, which occupy so conspicuous a place in the geology of the southern boundary of the valley of the Ganges and Jumna, appear to be continued on with but little interruption into the Bhurtpoor district, where they occasionally are found near the surface, though, generally speaking, they are hid from view by the alluvia of the Gangetic provinces. The latter in many cases seem to occupy extensive internal basins or valleys in this formation.

In the Bhurtpoor district, as elsewhere, the sandstones are characterized by the nearly horizontal position of the strata, and

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* The marble which was so extensively employed by the Agra architects during the reigns of Jehanghir and Shah Jehan, was brought from Makrana in the Joudpoor district; and appears to be a very durable stone. Unfortunately it was but too often associated in the same building with the more perishable sandstones. Shah Jehan, indeed, encouraged a chaster style of architecture; and the Laj Machal, where the ashes of this monarch now mingle with those of his beloved and beautiful Begum, is still preserved in all its pristine splendour. This magnificent mausoleum is, indeed, well suited to his gorgeous title of "King of the World." In the exquisite mosques for which the Laj is so celebrated, a variety of agates, jaspers, coloured marble, heliotropes, with lapes lazuli, plasma, and similar minerals, are employed; but I have seen none of the rubies and other gems which travellers have described as abounding in this edifice.

by the absence of enclosed organic remains. There are no partings interposed between the strata, the sections of the rocks exhibiting a succession of huge rectilinear tables, piled directly one on another. These strata are remarkably free from veins or fissures of any kind, and contain few, if any, embedded minerals.

The geology of the more recent sandstones of the Bhurtpoor district may be best studied at Rupbas, a town situated about thirty-two miles in a south-westerly direction from Agra. There are other quarries near the villages of Jugneer, Bussai, and Puharpoor, all of which places lie within a limited patch of country, distinguished by an undulating surface, and by several low rounded hills formed of the sandstones.

As far as I have been enabled to ascertain the fact, *there are no deposits of rock-salt or gypsum included in the rock formations of the Bhurtpoor district.* The soil, however, is impregnated to a great depth with saline particles, and a saline efflorescence very generally appears at the surface. From these sources are manufactured large quantities of a salt called *Kharee nimuk* (*i. e.* Bitter Salt, a name by-the-by very indefinitely applied to several saline compounds in which sulphate of soda exists as an ingredient), which is used by the natives as a condiment. The salts collected at the surface, together with a quantity of the saline soil, are washed with water from the brackish wells, and the solution thus formed is left to evaporate in shallow pits dug for the purpose. The pits are lined with a thin coating of lime, which is renewed after every deposition. The salt is deposited in cubic crystals, many of which are very perfect and colourless, though occasionally they are tinged with iron; they have a bitterish taste: the principal ingredient in their composition is chloride of sodium, which is combined with sulphate and carbonate of soda in small proportion, and a minute quantity of iron. A more accurate analysis I had not an opportunity of making.

The wells from which the salt-water is drawn vary in depth from 42 to 64½ feet, the richest water being that which is procured at the depth of from 51 to 60 feet. This last yields, according to circumstances, from a *chedam* to 1½ *pin weight* (*i. e.* from 1 to 3 per cent. by weight), and one well will produce in a season from 100 to 1000 *pullahs*, a *pullah*, or bullock-load being equal to 3½ *muns*, at 90 *sonat rupees* to the *seer* *. It is said that the water below 65 feet is also saline, but that the salt in this case does not form into separate crystals, but is left in a solid crystalline cake at the bottom of the evaporating pits.

The soil through which the wells are sunk is clayey, mixed with calcareous matter, fine siliceous sand, and scales of mica. It effervesces strongly with acids, the inferior beds are more tenacious and retentive than the upper, and beds of loose sand, exhibiting something of a stratiform appearance, occur at various depths below the surface. The principal salt manufactories are at Bhurtpoor, Deeg, and Kumbeer. From sources similar to the above, salt is manufactured in various other districts of India; and an inquiry into the phenomena attending its occurrence would be extremely interesting. Are such saline soils found only in connexion with rocks of the so-called Indian new red sandstone formation? Or, in other words, when sea-salt is found intermixed with the soils of the Gangetic provinces, is there any reason to believe that this circumstance has any connexion with the occurrence of rocks of the above formation beneath such soils? Or, on the other hand, have these saline particles been transported from a distance, in mixture with the other ingredients of the soils, and by the same causes which operated in forming the alluvia? Or, are they the produce of some chemical decomposition *still* going forward in the great laboratory of nature? The experiments of Sir James Hall have rendered this a most interesting subject of inquiry; and, supposing for a moment his theory relative to the influence of sea-salt as a consolidating agent to be correct, might we not also suppose that the salts now found intermixed with the soils had originally effected the consolidation of the strata below, and that, having passed through these strata in a state of vapour, the superjacent soils were thus impregnated with saline matters without themselves being consolidated? The latter circumstance might perhaps be attributed to diminished pressure from their superior position; and I can see nothing in the phenomena of sublimation to militate against such an idea. In this view of the case, the period of deposition of the diluvia must have been either synchronous with, or anterior to, the consolidation of the sandstone strata, and future observation perhaps may identify some of the lower beds of the loose arenaceous deposits with the superior beds of the new red sandstone formation containing rock-salt. Might not the identical agent, supposing it to have been a body of water concerned in the transport of the loose deposits, have supplied the pressure necessary for the conditions of Sir J. Hall's hypotheses? And might not the presence of so large an ocean in countries now far removed from the sea, be connected in some way with those great upheaving agencies which are supposed to have effected the elevation of our mountain masses? Well-marked indications of the operation of some such agencies may be perceived in the neighbouring hill ranges; and we might thus be enabled to trace a regular chain of phenomena which might all be referred to one grand original cause. The speculations of Elie de Beaumont, and others, have invested this subject with a degree of interest bordering almost on romance.

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* The weight of the *Sonat Rupee* is about 7 : 11 Troy; a *Mun* consists of 40 *Seer*.

Unfortunately, we know as yet but little of the minute geology of the Gangetic deposits, and the little we do know tends rather than otherwise to perplex the subject. The borings for fresh water now going forward in Calcutta, may probably lead to important results, and the attention of the scientific in India has of late been more forcibly directed than heretofore to such studies.

As far as I know, no organic remains have as yet been discovered in the deposits under consideration, except perhaps some recent shells found imbedded near the surface in the last formed of the *alluvial* soils. Had organic remains of the larger antediluvian animals been abundant, they must ere this have been observed on the banks of the Ganges, Jumna, and other rivers which have worn for themselves deep channels in the soils. From the loose nature of the materials forming them, these banks are constantly liable to extensive slips, and fresh sections, often of great perpendicular height, are thus daily exposed. Would the absence of organic remains, if satisfactorily demonstrated, tend in any degree to corroborate the idea of the identity, in regard to age, of the inferior beds of these deposits with the saliferous sandstone formations?

(To be concluded in next Number.)

Meteorological Tables, deduced from a Register of the Weather, kept at Bancoorah in the East Indies, during the Years 1827 *and* 1828. 4/5

During a residence of seven years in Bengal, I paid a good deal of attention to the weather, and kept a register as connectedly as I could for the greater part of the time. There is a remarkable similarity in the climate there, and I found that though the monthly results varied in some instances considerably, the yearly average did not materially differ. Thinking that it may be of some importance to know facts at a time when every thing connected with climate causes an unusual interest, I have sent you the results of two years which were considered to be extremes,—the one in respect of rain, and the other unquestionably of drought, in order that the medium of the two may be estimated as the weather commonly to be looked for at the place where I was stationed.

Bancoorah, situated in 23° 20′ North Latitude, and 87° 12′ East Longitude, is a civil station in Bengal, distant a hundred miles west-north-west from Calcutta, on the great road to Benares. The country is remarkably level upwards from Calcutta, until you reach Burdwan, a distance of fifty miles; and it is from this last place that the country ascends in a gradual elevation to Bancoorah, a distance of fifty miles, above which place the ascent is much more rapid, and the country becomes hilly. The face of the country about Bancoorah is covered with low woods, and the soil is gravelly, with a clayey sand on the surface, that becomes perfectly hard in the dry weather, and requires much rain or manual irrigation to render productive. The river Dalkissah, that passes Bancoorah, brings down in floods considerable pieces of trap and also of quartz rock, containing a large portion of mica (which is largely imbedded in the gravelly soil of Bancoorah and its neighbourhood), from the hills about its sources; but about the place itself, with the exception of two or three masses of quartz jutting above the surface, there is no rock or stone of any consequence. There is a considerable bed of coal and freestone upon the banks of the Damoodah river, about thirty miles north-east of Bancoorah; the coal resembles our Scots coal very much, and the freestone is durable, though coarse in its texture. Bancoorah is a very sultry place during the hot season of the year; but from its being situated above the low swamps, and in a great degree free from the noxious exhalations emitted from their surface, it is generally accounted to be the healthiest station in that part of India. It is elevated above the sea 215 feet, and above the tide-mark 160 feet, and at a distance of about 105 miles in a straight line north-by-west from the nearest sea-coast.

As the *temperature* in the accompanying table was noted from a thermometer placed against a wall in *the room*, it will be requisite that I should say something regarding the construction of the houses in India, so very different from those in this country. The houses there are all built of brick, flat-roofed, and thickly covered over and ornamented with finely pulverized lime wrought into plaster, that admits of a high polish if necessary. Every house has an open verandah or portico, with as many door-ways to each room as may be consistent with architectural proportion and stability, running along the whole length and breadth of the house, from the verandah and the outer-walls, to allow the external air, when refreshing, free access to the remotest corner. These door-ways are occupied by folding doors, with Venetian blinds in each half, which can be opened or shut as required, and glazed windows are hung in the same style immediately within these, so as to prevent the hot wind from blowing in upon you without darkening the room. In this manner the temperature of the room, into which the solar beams do not directly penetrate, is properly that of *the shade*, for on the outside of the house it is difficult to find a place free from the hot wind, and the direct or reflected rays of a vertical sun, which subjects the thermometer to sudden rises beyond the true temperature, as indicated in a room exposed to the heat of both without the direct influence of either. The external air, during the hot season, is insupportable without cover to the European; so that it is the house, under the foregoing circumstances, that he is the most partial to, and consents to be regulated by the temperature of his sitting apartment. But it is only whilst the hot winds continue that the two thermometers materially differ; at all other seasons of the year the temperature in both situations is remarkably equal. There was no artificial means resorted to, as *tattees*, for *cooling* the room in which my thermometer hung, so that the temperature is given as it really happened, without any influence upon it beyond the necessary openness of the dwelling. Tattees are very generally had recourse to for procuring comfort in the hot-weather. They are constructed of an oblong wooden frame of split bamboo, having the body of it thickly wattled with the small sprigs of the bamboo, and the interstices filled with the coarse grass common to the country. When wanted, this frame is placed against the door-way upon which the hot wind is blowing, with the folded-door shut, and the Venetian-blinds open, and being constantly kept wet with water, the wind passes through it *cool* into the room within. Whether tattees conduce to the *health* of those under their influence is a question left for medical men to determine.

The *atmospherical pressure* was noted from an excellent marine barometer in an adjoining room; and the quantity of rain was carefully measured by a rain-gauge placed in the centre of a grass-plot fronting the house, and away from all overhanging impediments. I did not observe the hygrometer. In the rainy season the atmosphere is perfectly moist, and in the hot weather it is dry to a very great degree, no dew falling during the night at this season. From what I observed, I was led to conclude that the driest month is April, and July that in which there is the most moisture. In the cold weather, the atmosphere is less dry than the clearness of the sky would indicate, from the heavy dew that falls during the night being evaporated by the succeeding day's sun, and remaining in a state of vapour, to condense again after his setting. The greatest range of the thermometer in the room was from 60° to 98°, and the greatest difference during the day never exceeded 9°, and that only following a severe storm, after a preceding temperature of 90° and upwards. I never saw the barometer lower than 29·250, nor higher than 30·200; and a variation of 2 lines between the two observations, was always looked upon as remarkable, and never happened but in very wet weather. The temperature of the external air in the cold season has been seen so low as 55° or 50° at sunrise: but this coolness only took place in a clear morning, after a fall of rain, for, when clouds made their appearance, there was no difference between the temperature of the external atmosphere and that of the room, although it might have amounted to 12° or 14° on the preceding morning at that time. The heavy dews that fall during the night, at this season, in clear weather, give a chilliness to the succeeding unclouded mornings more sensible to the feelings than a much lower degree of cold in more northern climates. The weather becomes warm in February; and the hot winds commence about the beginning of March, but seldom in *earnest* until the vernal equinox, and continue until the setting in of the rainy season in the first or second week of June. The hottest month is May, and the heat increases in sultry oppression as the rains approach, until their actual presence abates its insupportable tyranny. The heat of the night commonly exceeds that of the day in closeness for nine months in the year; and the most pleasant part of the twenty-four hours is an hour or two before sun-rise.

The rainy season in general sets in with heavy rain from the eastward, attended by severe thunder and lightning; the higher clouds' motion having previously come round by showers from different quarters to the point agreeing with the wind on the surface. The heaviest rain falls in July, and lessens in quantity until the middle or end of September, when the rains usually take their leave with a *flood* from the east, in a similar style to their commencement.

Solar and lunar halos are very frequent when the atmosphere becomes hazy and slightly overcast, but without that effect upon the coming weather expected in variable climates, excepting when it changes from dry to wet, and the contrary. *Lunar rainbows* are not uncommon in stormy showery weather. I had an opportunity of seeing three very perfect ones, in one instance so well defined as to appear double. *Parhelia*, with bright spots on and around the halos, are of general occurrence in the mares-tails and mackerel formation, which the clouds so often assume in an Indian atmosphere. *Eclipses* do not materially influence the weather: in 1828, the great solar eclipse of April was not followed by any alteration, with the exception of happening near to a periodical change of season, when they hasten its approach, as has been before remarked to be the only influential attendant on those phenomena. *Earthquakes* are generally attended with a hazy atmosphere, with small low motionless clouds, and the breeze on the surface blowing irregularly in strong eddies, with calms intervening.

It is in May and September that Cholera Morbus and other diseases prevail most. In the year 1825, the rainy season did not commence before the 19th of June, continued throughout very mild, and broke up in so light a manner as scarcely to be remarked at all. In consequence, the weather, for a month before their setting in, and at their conclusion, was unusually close and oppressive, although the temperature was not higher than that of other years at the times in question. The cholera raged in Calcutta in June, visiting the country here and there on that and the following months, and reached Bancoorah, among other places, in September, sweeping off a vast number of the natives in its way. After continuing for a fortnight or three weeks in the town of Bancoorah with fatal effect, almost in every case baffling the power of medicine, it all at once left the place and appeared in Chatna, a village about eight miles to the northward. In this manner it travels through the country in continued hot moist weather, carrying off great numbers of the native population, and, I believe, in some instances taking one portion of a village, or even one side of a street, leaving the rest of the village untouched in its progress onward. In Calcutta, the cholera is among the natives in the Black Town almost at all seasons; the low situation of the city, and the crowded and narrow streets of the native part of it, being exceedingly well suited to keep the disease from abating even during the coolest part of the year. At this time Calcutta is almost daily visited with a dense damp fog hanging over the city and the River Hoogly in the morning and evening. In May 1828, the cholera was not only mortal to the natives in that city, but there were more numerous instances of its proving fatal to Europeans there and in other parts of the country than former years had exhibited. This might have arisen from the unexampled heat of that season. There cannot be a more impressive case of distress than that of witnessing a patient labouring under Cholera

Morbus. In the morning he might have been perfectly well—before sun going down (a speedy interment is in that climate indispensably necessary) the tomb covers all that is mortal of him! From the great evaporation that takes place before and after the departure of the rainy season, giving an intolerable closeness to the heat, September and the first weeks of October have been always considered the most unhealthy season of the year. The cold weather is not generally thought to be the time most conducive to health, from the coolness of the air giving a check to perspiration, and the heavy dews that then fall at night proving so damp and chilly as to make the least exposure to them be attended with prejudicial consequences. The season commonly considered to be the most healthy is the continuance of the steady north-west hot wind, when a copious perspiration is produced and speedily evaporated off the surface of the body by its warm dry influence, giving a light cheerfulness to the spirits they never can have under the pressure of a sultry, still, and close atmosphere.

GEORGE MACRITCHIE.

CLUNIE, 12*th March* 1832.

To R. JAMESON, Esq.
Prof. Nat. Hist. &c.

TABLE shewing the Temperature and Pressure, and the quantity of Rain fallen, during the years 1827-8.

1827.	Monthly average temperature at 10 A. M.	Monthly average temperature at noon.	Monthly average temperature at 10 P. M.	Monthly average pressure at noon.	Fall of rain in inches.
				In.	
January . . .	66°.75	68°.25	67°.50	29.800	1.729
February . . .	73.25	74.50	73.50	29.800	
March	78.25	80.50	78.25	29.775	1.199
April	83.00	84.25	83 25	29.750	1.336
May	86.00	87.25	86.00	29.750	1.765
June	84.50	85.50	84.25	29.525	9.479
July	83.50	84.00	83.25	29.350	13.087
August . . .	81.50	82.25	81.75	29.425	11.676
September . .	81.50	82.00	81.25	29.525	12.678
October . . .	80.75	81.75	80.25	29.700	.584
November . .	73.75	74.75	73.00	29.800	.217
December . .	68.75	69.75	68.50	29.825	.478
Yearly average	78.46	79.56	78.39	29.669	Total } 54.228
1828.					
January . . .	66.75	67.50	67.00	29.825	1.679
February . . .	69.50	70.50	69.75	29.800	.902
March	79.50	80.75	79.25	29.900	
April	86.75	87.50	87,75	30.050	.003
May	90.00	91.00	90.00	30.075	.144
June	86.50	87.50	86.00	29.750	7.368
July	82.75	83.50	82.50	29.425	8.698
August . . .	83.00	83.75	82.75	29.450	5.682
September . .	82.50	83.00	81.75	29.550	7.272
October . . .	80.25	80.75	79.50	29.700	3.166
November . .	75.50	76.00	74.00	29.825	.601
December . .	69.75	70.50	68.50	29.800	
Yearly average	79.39	80.19	79.06	29.762	Total } 35.515
Average 1827 . ……… 1828 .	78.46 79.39	79.56 80.19	78.39 79.06	29.669 7.2962	Aver. } 44.871
Medium for the two years . }	78.92	79.87	78.72	29.715	

TABLE shewing the General Direction of the Winds, and the number of Days in which each prevailed.

1827.	Days from N.	Days from NE.	Days from E.	Days from SE.	Days from S.	Days from SW.	Days from W.	Days from NW.
January . .	1	3	...	...	...	4	6	17
February . .	...	...	...	1	...	5	13	9
March . . .	...	...	...	...	...	4	15	12
April	...	...	...	...	...	3	17	10
May	1	1	1	2	...	3	14	9
June	...	5	2	8	3	9	2	1
July	2	1	4	2	5	1	7	9
August . . .	...	3	3	9	4	5	4	3
September . .	...	...	11	7	...	2	4	6
October . . .	4	3	...	1	...	...	11	12
November . .	10	...	...	1	...	...	7	12
December . .	8	1	...	...	...	4	3	15
Number of days	26	17	21	31	12	40	103	115
1828.								
January . .	6	1	...	...	...	5	10	9
February . .	5	...	...	1	1	1	12	9
March . . .	3	...	...	...	...	1	11	16
April	1	...	...	...	...	3	15	11
May	...	...	...	...	1	6	9	15
June	3	2	2	2	5	7	6	3
July	1	2	12	3	2	4	6	1
August . . .	2	2	7	5	6	4	3	2
September . .	2	1	7	8	8	...	2	2
October . . .	9	3	2	8	...	4	2	3
November . .	16	5	...	...	...	...	1	8
December . .	7	4	1	...	...	2	4	13
Number of days	55	20	31	27	23	37	81	92
Average number of days .	40½	18½	26	29	17½	38½	92	103½

N. B.—Under NE. is included all the intermediate points from N. to E.; and the same with regard to SE., SW., and NW

TABLE shewing the Phenomena of the Weather kept at Bancoorah during the years 1827–8.

27.	Days clear sunshine.	Days clear sunshine, with clouds.	Days sunny; sun wading.	Days cloudy.	Days in which any rain fell.	Solar halos.	Lunar halos.	Mock suns.	Mock moons.	Lunar rain-bows.	Earth-quakes.	REMARKS.
an.	6	15	3	7	7	...	...	...	...	...	2	On the 17th at ¼ to 7 P. M., and on the 19th at ½ past 11 A. M., at both times shock very sensible.
eb.	7	17	4	...	...	3	1	...	...	...	...	
ar.	...	19	9	3	8	...	2	...	...	...	...	
pr.	...	19	11	...	14	4	3	...	...	...	...	Solar eclipse on the 26th at ½ past 7 A. M.
ay	...	16	15	...	11	1	1	1	...	...	...	Bright meteor on the 2d at 7 P. M. The Cholera Morbus, Small Pox, and other diseases raging with great virulence during the latter part of this month, and first days of June.
une	...	3	18	9	23	2	5	4	...	...	...	The sun's rays at setting on the 22d, beautifully arched across the zenith, meeting in a point in the eastern horizon.
July	...	...	22	9	29	4	3	1	...	...	...	
Aug.	...	2	17	12	26	5	2	...	...	...	...	
Sept.	...	12	8	10	18	4	4	1	...	...	...	The sun's rays at setting on the 2d assumed the same appearance as above remarked on the 22d June.
Oct.	4	24	2	1	3	...	3	1	...	...	...	
Nov.	5	22	1	2	2	1	...	...	...	...	...	Lunar eclipse on the 3d at ¼ to 11 P. M. Bright meteor on the 6th at ¼ past 6 P. M.
Dec.	2	23	6	...	1	...	...	...	...	...	...	
Number of days.	24	172	116	53	142							
1828.												
Jan.	5	13	12	1	6	...	2	...	...	...	...	
Feb.	6	13	6	4	5	...	...	...	...	...	...	
Mar.	14	9	7	1	1	...	...	...	...	...	...	
April	3	13	14	...	5	1	...	...	...	...	...	Grand Solar eclipse on the 14th at 5 P. M., 10½ digits of the disc eclipsed.
May	8	17	6	...	7	...	3	...	...	...	...	Cholera Morbus very mortal during this month. An unusual heat or moisture in the climate for any length of time is the season for this malady being most deadly.
June	...	8	16	6	18	3	7	4	...	...	...	
July	...	2	23	6	26	1	5	2	...	1	1	On the 8th at ½ past 2 A. M. The Lunar rainbow on 28th at ¾ to 10 P. M.
Aug.	...	1	24	6	28	3	...	...	...	...	1	On the 23d at 11 P. M., attended with a loud rumbling noise from the northward.
Sept.	...	4	21	5	20	2	5	...	1	..	2	On the 15th at 7 P. M., and on the 18th at 7 A. M. The mock moon on the 23d at ½ past 7 P. M.
Oct.	...	19	10	2	13	2	...	...	...	...	...	
Nov.	1	20	6	3	3	...	3	...	...	...	...	Vivid meteor on the 6th at 6 P. M.
Dec.	7	18	5	1	2	.	...	...	...	...	...	
Number of days	44	137	150	35	134							
	34	154½	133	44	138							

On the Graphite or Black-Lead of Ceylon.

SEVERAL years ago, splendid specimens of graphite were presented to me by one of my pupils, who brought them from the island of Ceylon, where, he informed me, the mineral occurs in masses varying in magnitude from the size of a nut to several inches in diameter, imbedded in gneiss. Mr J. Prinsep has lately, in the "Gleanings of Science," an interesting periodical published at Calcutta, published the following analysis of one of the varieties: Carbon 62.8, iron 5.4, silica 21.0, alumina 9.3, lime 0.2, magnesia 0.1, manganese a trace, and loss 1.2, = 100. Suspecting, from the large proportion of earthy impurity in this analysis, that some of the matrix had remained mechanically mixed with the graphite, Mr Prinsep made the following additional analyses: 1. The graphite, uncleaned, left, as above, iron and earth, per cent. 37.2. 2. Roughly cleaned, left a residue per cent. 18.5. 3. Crystals selected with care, 6.0. 4. Another trial left the very small proportion of 1.2. The two last residua did not entirely dissolve in muriatic acid; indeed the former yielded 0.3 of silica on analysis. This statement is illustrative of the accuracy of Karsten's view of the composition of graphite, namely, that it is a mere modification of carbon, and the iron and earths are accidentally mixed parts*. The graphite of the Himala Mountains yielded to Mr Prinsep the following ingredients: Carbon 71.6, iron 5.0, silica 15.0, alumina, &c. 8.4 = 100.0. The English graphite was found by Mr Prinsep to yield, hygrometric moisture 2.7, carbon burnt off with difficulty 53.4, iron taken up by acid 7.9, earthy impurities 36.0 = 100.0. As this variety was marked of "superior quality," it follows that, chemically considered, it is inferior to the kinds found in Ceylon and the Himalas.

The Ceylon graphite, Mr Prinsep informs us, has only been known commercially for five or six years; the government had shipped small quantities of it to England by way of trial, and it answered so well, that they were induced to receive it, amongst other articles, in lieu of revenue, at a fixed valuation, when they were suddenly surprised at the quantity of this novel currency offered in payment. A large heap was thus accumulated; and as the island abounds in this mineral, and there are no padlocks upon the mines, as in Cumberland, it might soon effectually destroy the income of the Borrowdale Company, if introduced largely into the English market. The natives of Ceylon make no use of it.

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* *Karsten's* interesting experiments on graphite will be found in one of the *volumes of* this Journal.

Analyses of several Indian, Chinese, and New Holland Coals.

By J. Prinsep, Esq. Secretary to the Physical Class of the Asiatic Society of Calcutta.

The following table, published in the Calcutta Gleanings of Science, comprises the results of several analyses of Asiatic and New Holland coals. The fourth column, containing the water expelled, is kept distinct from the three which follow it, under the head of composition, as it is usual to include all the volatile products together. Should the water be looked upon as hygrometric, the per-centage of carbon and ashes must be increased to obtain the true composition of the coal: thus the Baghelpur slate-coal, after deducting 10 per cent. of water, contains

Volatile matter, $22 \times \frac{100}{90} = 24.4$
Carbon, . . $40.5 \times$ do. $= 45.0$
Ashes, . . . $27.5 \times$ do. $= 30.6$
100.0

From the last column in the table, it will be seen how totally unfit are most of the Indian coals for the purpose of making coke. The Burdwan coke, with the exception of one specimen, would contain nearly a quarter of its weight of earthy impurity; the Silhet would be still worse; the anthracite of Baghelpur would be nearly half earth; some of the mountain coal from Ava would yield a coke of better quality, but of very little density. The Chinese glance-coal alone forms a remarkable exception to this unfavourable conclusion against oriental coal, and deserves to rank at the head of the list in respect to its purity as a coke, although in specific gravity it does not come up to the character of the English fuel, neither has it the spongy texture which must contribute much to the glowing combustion of the latter. It will be remarked, that the ashy residue on the analysis of English coke (No. 2), much exceed what should have been expected from the composition of the coal whence it was formed; this may be explained by supposing that portions of the ashes, probably the alkaline salts, are volatilized along with the gaseous matter, when suddenly decomposed at a high temperature; or that considerable variation exists in the quality of the material charged in the oven. Whatever may be the cause, the same deterioration might be looked for in the coking of Indian coal, which would tend to lower them still more in scale of comparison.

TABLE of Indian and other Coals analyzed at the Calcutta Assay Office.

No. of specimen.	Denomination.	Specific gravity.	Water expelled on sand-bath.	Volatile matter, including water.	Carbon.	Ashes.	Per-centage of ash in the coke made from each coal.
1	English pit-coal,	1.273	1.5	29.0	69.4	1.6	2.3
2	Ditto, on the large scale,	...	...	25.0	73.0	2.0	2.6
3	New South Wales coal (average)	1.277	3.3	38.0	60.1	1.9	3.1
4	Burdwan coal,	1.334	8.0	39.5	45.9	14.6	24.0
5	Ditto, another specimen,	...	8.2	41.5	45.2	13.3	22.5
6	Ditto, (from China-Kúri)	1.340	8.0	32.5	61.1	6.4	9.5
7	Manipúr coal, Tank-Kiouk,	1.361	6.2	39.3	49.7	11.0	18.1
8	Towa or Hoshangabad ditto,	...	?	27.0	58.0	15.0	20.5
9	Silhet brown coal, (from Laour)	1.398	10.1	44.4	41.1	14.5	26.1
10	Ditto, lighter colour, slaty,	1.380	2.8	58.8	28.6	12.6	30.5
11	Ditto, soft friable bituminous,	1.348	7.1	42.2	41.0	16.8	29.0
12	Kasya hills, (Chirra-púnji), best,	1.310	7.0	37.1	62.0	0.9	1.5
13	Ditto, slaty,	1.520	12.3	38.4	53.4	8.2	13.3
14	Ditto, brown friable,	?	36.0	63.6	29.2	7.2	20.0
15	Palamú slaty coal,	1.482	9.1	37.4	52.1	10.5	16.8
16	Ditto, without lustre,	1.419	7.1	36.4	54.1	9.5	14.9
17	Warda nala Anthracite *,	1.457	7.8	43.8	33.7	22.5	40.0
18	Baghelpúr ditto *,	1.540	10.0	32.0	40.5	27.5	40.4
19	Sohagpúr ditto *,	...	6.0	25.0	29.0	46.0	61.4
20	Silhet bituminous shale,	2.042	...	22.0	26.0	52.0	66.0
21	Ditto, (Chirra-púnji)	2.187	6.3	23.0	6.6	70.4	91.4
22	Ava jet coal (Kuenduen river)	1.363	8.0	40.0	54.1	5.9	9.9
23	Ditto, lignite,	1.276	5.0	54.5	43.0	2.5	5.5
24	Himalyan ditto,	1.343	?	51.0	40.2	8.8	17.9
25	Ditto, dull,	1.458	21.0	56.0	37.5	6.5	14.8
26	Travancore fossil seeds,	...	?	52.0	45.0	3.0	6.2
27	Chinese glance-coal,	1.282	3.0	7.0	91.6	1.4	1.5
28	Ditto earthy, blind coal,	1.888	0.9	7.0	79.3	13.7	14.7
29	Coke from English coal,	1.600	.0	2.0	91.5	6.5	6.7
30	Coke from Burdwan coal (China-Kuri)	0.820	.0	.0	97.0	21.0	21.0

* The coals above enumerated as anthracites are evidently bituminous coals.—EDIT.

On the Fossil Flora.

The fact that we do not know more than a few hundred species of fossil plants, shews the impossibility of our pretending at present to form any very plausible estimate or conception of the nature of the antediluvian flora, and also the hazard of attempting, from the disappearance of a few species, in proceeding from one formation to another, to determine the abundance or paucity of families or classes of plants. It is surprising to observe naturalists characterising botanically whole antediluvian epochs, when they possess, as in the case, for example, of the second epoch, the period of formation of the new red sandstone, a group of twenty miserable remains, as the whole of the disposable riches from which such a result is obtained. It is evident also that we cannot, from the present fossil vegetable remains of single families, form any satisfactory inference as to the number of their species, because it may have depended in part on accident, or some unknown circumstances, that in one family many species and individuals are preserved, while in another but few remains are met with. In other cases the circumstances, although easily understood, are not attended to, and the number of fossil species found in the deposit, are considered as the full measure of what has been destroyed. Thus in the first epoch two mosses only, but sixty-four lycopodiæ, are enumerated; but this circumstance does not prove that mosses and lycopodiæ formerly existed in the proportion of 2 to 64; for it is very natural that gigantic lycopodium stems would more easily resist the storms of time than the dwarf moss; and although we on that account have only two mosses remaining from that period, it does not follow that they have not been more than ten times more numerous than the lycopodia.

It is doubtful if botanists can determine with accuracy even the family of the fossil species. Many of the remains at present considered as Lycopodiums, may prove to be Ferns, or even Pines, the Selaginites excepted. Many of the gigantic, so called Equiseta, resemble tree-like grasses. The Marsileaceæ of the former world may have belonged to the ferns. The *Voltzia* has not yet been proved to belong to the Coniferæ, and as the confirmation of the coniferous character will give a decided preponderance to this class of plants, in the period in which it occurs, we see from this example how uncertain single conclusions are, which have been considered as sufficient in regard to the prevailing character of many vegetable epochs. An error, which can be so easily committed, in endeavouring to determine fragments very difficult of determination, is doubly prejudicial, when we attempt to found on it a history of the gradual succession of the families of plants. The whole study of the ancient flora,—the flora of an early world,—is based on such a system of the history of vegetation, and on the proofs that it rises gradually from below upwards in regular successive epochs of more and more perfect vegetables. But in order to judge of this history of vegetation, it is premised that we have previously ascertained the true succession, the internal gradation of vegetation, in short the true natural vegetable system. But who has discovered this system, and what are its characters?

Notice by Dr GRAHAM *of Botanical Excursions into the Highlands of Scotland from Edinburgh this season.*

THE growing attention which has been given here to botanical science, has been evinced in many ways of late years, and, among others, by the increasing desire to extend the sphere of observation from the Botanic Garden to the neighbourhood of Edinburgh, and thence to the more distant and alpine regions of Scotland. In my first excursion into the Highlands from Edinburgh in 1821, I was accompanied by Mr Macnab alone, and joined by Mr Mylne and Mr Drummond from Forfar. In 1825, I walked round the west and north of Scotland accompanied by one pupil only, Mr Home. In 1827 I walked over nearly the same ground with eight or nine pupils. Every year since, the party has been quite as large as the accommodations were at all adequate to receive; and in the last three years, I have been favoured with the company of friends, all of whom have added greatly to the pleasures of the party, and the acquaintance with botany which several of them possess, has greatly increased the *means* of exploring the wide extent of the only half-examined mountain-ranges of Scotland. My academical duties necessarily detain me in Edinburgh till the close of the summer session at the end of July; but the impatient zeal of some of my most elastic friends could not be compressed so long this year, and, exploding during the summer heat, carried them to Forfar on the 12th of July. A great additional interest has been given to the excursion of this party, by the barometrical observations of my excellent, intelligent, and quietly persevering friend Mr Hewett Watson.

After a hasty glance at the neighbourhood of Forfar, they proceeded to Kirkton, Clova, spending five days in that neighbourhood. The weather among the mountains was excessively rainy and foggy, and it sometimes blew so violently, that they felt it dangerous to venture among unknown precipices. Several attempts were made to explore new ground, in which they lost their way and each other, and, upon one occasion, after walking for hours, they met unconsciously near the spot from which they had set out. They therefore generally contented themselves with gathering the rare plants from the already known stations, and then proceeding by the same route as last year, along the White Water to its source, and through Glen Callader, reached Castletown of Braemar. From this they visited the mountains around; but here also the influence of their evil stars chased them, chilled them with a snow shower on Lochnagar, pelted them with rain on Ben-na-buird, and buried them in the mists of Ben-Avon. No wonder if such treatment cooled their zeal. The party broke up here: three crossed Cairngorum and Ben-na-muic-dui to Aviemore and Inverness; the others went off in various directions, leaving Mr Watson alone to continue his botanico-barometrical observations. He also subsequently proceeded to Inverness, and thence along the coasts of Caithness and the north shore of Scotland as far as Erriboll, in spite of continued rain and dense fog; but finding himself an object of extreme suspicion, from having come from a country infected with cholera, he delicately abstained from presenting his letters of introduction, and, leaving the country, returned to Edinburgh, by Inverness, the Caledonian Canal, Glencoe, Killin, and Stirling.

The party with which I had the pleasure to be accompanied, left Edinburgh on the 28th of July, and proceeded directly to Kirkton, Clova. We remained there till the 7th of August, when part returned to Forfar, while the rest went to Glen Isla, and thence we proceeded on the 9th across the mountains directly to the station of *Carex Vahlii*, at the head of Glen Callader, and passing down the glen, reached Castletown. Here our short but most interesting excursion terminated: I was obliged to set off for Edinburgh early next morning. Our fortune in regard of weather was the very reverse of that of the party which had preceded us. It was hot, calm, and, without an exception worth noticing, fair. We were therefore enabled to adhere to our resolution to devote almost our whole time to visiting new ground; and the only old stations which we went to were Loch Brandy; the birch trees at the top of Glen Dole; the rocks on which the *Astragalus alpinus*, the *Oxytropis campestris*, and the *Carex Vahlii* severally grow. Among the fruits of this resolution exhibited in the following list, I would particularly point to a profusion of *Lychnis alpina*, not seen before, except by Mr Don; to five new stations for *Sonchus alpinus;* to abundance of *Gentiana nivalis*, never before found in Britain, excepting on Ben Lawers, and there only sparingly, and at long intervals, by two or three lucky botanists; to *Thlaspi alpestre*, new to Scotland; to the frequency of *Malaxis paludosa*, in various stations; and to *Carex rariflora*, gathered to satiety in all the bogs at a particular elevation on one range of mountains, though, before this season, two stations only were known for it. These discoveries are accompanied with a consideration which multiplies many times the pleasure which I derive from them. The late indefatigable Mr DON of Forfar, by his unceasing researches among the Clova mountains, made many, little expected, additions to the British Flora. Doubts were stated as to these being really indigenous, and explanations, the reverse of charitable, were offered of their appearance in places which Mr Don was wont to visit. That a plant stated to be noticed by Mr Don had not been re-discovered after his death, was not a reasonable ground for doubting his good faith; but it cannot be denied, that colour was given to these suspicions by the re-discovery of *Sonchus alpinus* in one station only, without a seedling appearing, though the station was such as seemed to insure the dispersion of its seeds if it could ripen them. Mr Drummond, I believe, knew more stations; but no other living botanist had seen this plant growing in Britain beyond the limits of one small shelf of rock. We saw it in five new stations, two of them more than two miles from the original spot; and not one person among us has the slightest doubt of the plant being really indigenous. When we know this,—when we see it as certainly proved that *Lychnis alpina* is indigenous,—when we recollect the discovery in Glen Callader, during our excursion two years ago, of *Carex Vahlii*, and of *Astragalus alpinus* in Clova last year, and this season of *Thlaspi alpestre* and *Gentiana nivalis* in Glen Isla, and of *Carex Vahlii* in Clova,—when we see how wide the range of mountains is, and how very circumscribed the stations of all the remarkable plants in the district, so that the last four plants I have mentioned had escaped even the practised eye of Mr Don, I do think it would require a much stronger case than has yet been made out to shake the credit of Mr Don for perfect sincerity *.

I shall now give a list from both parties of the plants noticed by them, and not robbed of their interest by the account given of former excursions; marking the observations of the first and second parties by the figures 1. and 2. added to the names of plants. In conclusion, I shall give, in his own words, the observations kindly communicated to me by Mr Watson, regarding the elevation at which he found plants to grow in different parts of his route.

Acinos vulgaris, 1.—Dry pastures eastward of Forfar.

Ajuga alpina, 1, 2.—This plant was seen in large quantity, particularly above Bachnagairn and in Glen Isla. After having been gathered by some of the first party, it was thrown away by them, as not different from *A. reptans*. I fear they were right; for, though it is certainly the plant figured in Eng. Bot. as *A. alpina*, and though there is a very slight difference in habit from the ordinary low country form, I cannot observe any real specific distinction. It produces abundance of stolons, though not on every plant.

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* We have always expressed our indignation on hearing the hints thrown out against the veracity of the late Mr Don, whose honour was unimpeachable; and who, as an acute, active, and successful observer and collector, was fully equal to any of the Scottish botanists of the present time.—Edit.

Alisma ranunculoides, 1, 2 —Marshes near Forfar.

Apargia Taraxaci, 1. 2.—Frequent on the table-land in the upper district of Clova. I cannot see any good specific distinction between this and *A. autumnalis*, and I think both pass into the *A. alpina*, which I found in 1825 and 1827 on the mountains in Sutherlandshire, and in the present station last year.

Arabis petræa, 1.—Near the summit of Ben-na-muic-dui, and at its base, where it had been brought by the streams, and where we observed it to have established itself in 1830.

Arbutus alpina, 1.—Found by Mr Watson on Ben Shith (so written, for some good reason, I dare say, but pronounced Ben Hee), Ben Loyal, Ben Hope, and the moors about Loch Erriboll, in Sutherland; Ben Nevis, and Cairn Garidh, Inverness-shire; and the hills to the north of Loch Eil, in Argyleshire.

Astragalus alpinus, 1, 2.—This was only found in the station where it was first recognised, and seen abundantly last year. The station of *Oxytropis campestris* was most carefully examined for it by both parties, but not a bit of it was found. Mr Watson is now therefore disposed to think that his belief that he saw it there last year originated in a mistake, and consequently we only know one station for the species.

Carex aquatilis, Wahl. 2.—Very abundant in bogs on the extended table-land in the upper district of Clova. This *Carex* has been many years known in this station, and particularly was first remarked as peculiar in its appearance by Mr Watson in our excursion last year; but though its characters did not well agree either with *C. acuta* or *C. stricta*, it was hesitatingly referred to one or other of these. On our return to Edinburgh this season, Dr Greville found in his herbarium a specimen from Fries, under the name here adopted. It is undoubtedly identical with the Clova plant, and agrees in all material points so well with the character in Flora Lapponica, that I do not hesitate to agree with Dr Greville that thus another *Carex* may be added to the British Flora. It scarcely ever exceeds a foot and a half in height, and is often much less; whereas Wahlenberg describes his plant as sometimes nearly equalling the height of a man. In lower ground, and in sheltered situations, I entertain no doubt that our plant would acquire a much larger size. We never observed it below the table-land, though it reached the edges of this in almost every direction, filling the bogs in many places with its creeping roots, and visible at a distance by its large foliaceous bracteæ, rigid slightly curved aspect, and pale green colour. I ought to add, that the plant distributed by the Unio Itineraria, as *Carex aquatilis*, is altogether different from ours, and seems to me nothing but *C. cæspitosa*.

—— *atrata*, 1, 2.—Found in great abundance on almost every cliff we visited, particularly on the south side of the Fee, and in Glen Isla.

—— *pulla*, 1, 2.—We saw this only in one station, on wet places among the rocks on the south side of the Fee. Dr Wight first gathered it, of such unusual size (nearly two feet high), and in all respects having corresponding proportions, that it was with difficulty I could believe in the identity of the species, though the characters perfectly agreed, till I visited the spot, and found amongst the larger specimens others gradually coming down nearly to the size I had found it on other mountains. Mr Watson alone of the first party found this species, not in Clova, but scarcely of its usual size, on Cairn Garidh, near Ben Nevis.

Carex rariflora, 1, 2.—The first party found this hitherto scarce *Carex* in one new station, we at least in a dozen; and there is not a doubt that it exists in the utmost profusion in almost every bog on the table-land.

—— *Vahlii*, 1, 2.—Mr Brand, who accompanied the first party, had the good fortune to find this plant in a new station on the south side of the Glen of the Dole, near its lower extremity. We only gathered it in the old station at Glen Callader.

Cicuta virosa, 1.—Forfar Loch.

Cochlearia officinalis, var., 2.—Leaves beautifully variegated with broad white edges, or entirely white. I found only one patch of this on moist rocks on the south side of the Fee. The plant was remarkably vigorous, and the leaves very large.

Dryas octopetala, 1, 2.—On the cliff with *Astragalus alpinus*, in Glen Isla, in the parish of Farr, and most abundantly in the parish of Durness, where it is confined to limestone rock.

Epilobium angustifolium, var. *angustissimum*, 2.—Dr Greville found this in the descent to Glen Isla. It had a very peculiar appearance, was not in flower, but seemed distinguishable only by the extreme narrowness of its leaves, varying from 2 to 4½ lines.

Equisetum Drummondii, 1.—In a ditch by the road side, about four miles north of Forfar, and on the banks of the Caledonian Canal, near its east end.

Erigeron alpinus, 1, 2.—Both in Clova and Glen Isla, but especially in the latter, where Dr Wight gathered a few specimens with two flowers.

Galium pusillum, 2.—I gathered this sparingly near the *Oxytropis campestris*.

Gentiana nivalis, 2.—Found in abundance at a moderate height upon the rocks on both sides of Glen Isla by Dr Greville, Dr Wight and myself, varying from a simple stem, scarcely a quarter of an inch high, to one greatly branched, and 6 inches in height, but always with flowers of equal beauty, and not differing proportionally in size. The sparkling of this most rare and lovely little gem among the scanty mountain herbage, cured me of hunger and thirst, and made me forget that I was gathering it at the risk of my neck, for which I have in general on such occasions a regard at least equal to its value.

Goodyera repens, 1.—Abundantly in old fir woods at Cawdor Castle and at Gordon Castle.

Linnæa borealis, 1, 2.—Mr Watson first found this in vast profusion, and covered with flowers, among the heath at a considerable distance from any trees, on the south side of the Glen of the Dole, and a little way from the stream.

Lychnis alpina, 2.—This very interesting plant, not before found in Britain, except by Mr Don, was gathered by Sir John Ogilvy on the 30th of July. We went to the station next day, and gathered it in abundance, and in full flower. The locality is circumscribed, but Mr Macnab afterwards observed it in smaller quantity on another spot, at a little distance on the same mountain. It grows on a bare, dry, stony summit, or in the edges of the immediately adjoining peat, at an elevation which, judging from the heights ascertained by Mr Watson, I suppose may be about 3200 feet above the sea. The rock in both stations is a mixture of felspar and talc, is much weathered, has a deep cream colour, and is quite different from any thing around. We observed that in the same spots grow *Armeria vulgaris*, *Cochlearia officinalis*, and *Cherleria sedoides*, and there only at this elevation. A shepherd whom we met upon the ground told us he had seen the plant growing on a mountain top, either between the Glen of the Dole and Bachnagairn, or between this and Lochnagar, I am uncertain which, because, as he evidently did not distinguish between the *Armeria* and *Lychnis*, I paid little attention to his report at the moment; but reflecting since on the one never having been seen without the other on these mountain tops, I think the shepherd's accuracy should have been inquired into. The capsule of *Lychnis alpina* is defined as unilocular. Our plant, which is certainly that of Linnæus, as I learn from Mr D. Don, who has compared it with the Linnean herbarium, is most distinctly 5-locular when far advanced to ripeness. Whether the dissepiments ultimately disappear, whether the Swiss plant is in this respect different from ours, or whether the description of *its* capsule be incorrect, I am unable, from want of ripe fruit, to determine.

Malaxis paludosa, 1, 2.—This we found in many situations, and in large quantity, in the bogs and by the sides of the rills in the valley of Clova, or a few feet up the mountains on both sides.

Molinia depauperata, 2.—Extremely vigorous, and in large quantity, on the cliff near the *Oxytropis campestris*.

Nasturtium terrestre, 1, 2.—Not a common plant in Scotland, but found by the first party near the Loch of Forfar, and by Dr Wight near Lochleven.

Nuphar pumila, 1.—In various stations.

Poa alpina, 2.—In large quantity in Glen Isla, and not in a viviparous state.

Potentilla alpestris, 2.—Widely distributed on the rocks to the south of the Fee, and in Glen Isla.

Primula scotica, 1.—Found by Mr Watson in various stations from Thurso to Farr. Some of the specimens have several long single-flowered scapes, the same variety of form which separates *Primula vulgaris* from *P. elatior*.

Pyrola rotundifolia, 1, 2.—Very sparingly in Glen Dole.

——— *secunda*, 1, 2.—Much more common than the last on rocks in various places in Clova.

Salix lanata, 1, 2.—Both male and female plants in great perfection on many rocks in Glen Dole, Glen Isla, and Glen Callader.

—— *rosmarinifolia?* 1.—Probably from the same bush as that seen by Dr Greville two years ago, and in no better condition.

Saxifraga cæspitosa, 1.—Picked by Mr Barry, but sparingly, in the same situation as that in which it was seen by him last year.

—— *nivalis*, 1, 2.—In tolerable quantity in Clova, but much more abundantly, and in better condition, in Glen Isla.

Salix rivularis, 1.—On the old stations on Lochnagar and Ben Nevis.

Schœnus nigricans, 1.—Various moors in the north of Sutherland.

Sedum album, 1, 2.—Roofs in Forfar, where it was introduced by Mr Don.

Silene acaulis, var. flor. *alb.* 1.—In considerable quantity on the summit of Ben-na-muic-dui, and on the south side of Glen Dole.

Sonchus alpinus, 1, 2.—Found in five new stations in Glen Dole and Glen Isla by Dr Wight, Dr Greene of Boston, U. S., and Dr Greville. The first party found it only in the old station, where, from being constantly plundered, it is becoming weaker every year. Fortunately some of the new stations are wholly inaccessible.

Stratiotes aloides, 1.—In the Loch of Forfar, where it was introduced by Mr Don.

Thlaspi alpestre, 2.—New to Scotland. I gathered a few specimens at the foot of the rocks in Glen Isla.

Veronica alpina, 1, 2.—Exceedingly common all over the banks of the White Water, in Glen Dole, upon the Fee, in Glen Isla, Glen Callader, and the Aberdeenshire mountains near Castletown. It is not common in the other parts of Scotland which I have visited. Mr Watson found a few specimens on Cairn Garidh.

—— *saxatilis*, 1, 2.—Common in Glen Dole; but in much larger quantity, and of much greater size, in Glen Isla.

Woodsia hyperborea, 2.—A single tuft was found by Dr Greville in Glen Isla.

—— *ilvensis*, 1.—Found by Mr Watson sparingly on the cliff with *Oxytropis campestris.*

Observations of Mr Hewett Watson *.

ABSOLUTE altitude is of so little importance in the geography of plants, that my attention was for the most part limited to the observation of their relative heights in regard to each other. For this purpose, however, it was necessary to determine the heights of a few species most commonly met with, as points of comparison and reference. The following are the averages of various observations made near the places mentioned; the numerals denoting the altitudes above the sea-level in feet †.

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* We trust Mr Watson will continue his interesting investigations, and in his next communication inform us how his heights of stations were ascertained.—EDIT.

† *Arbutus alpina* does not grow in Clova or Braemar. Its southern limit in Britain is probably near Fort William. There its lowest limit (average of three observations, several miles distant) is at 1970 feet; on the north side of Ben Shith (pronounced Ben Hee), near Tongue, 970, and by Loch Erriboll, fourteen miles west of Tongue, about 250. (These last measurements give a good example of the influence of longitude, particularly when taken in conjunction with the descent of *Thalictrum alpinum* to the shores of Keoldale, a few miles to the westward of Loch Erriboll, as noticed in the excursion in 1827. Mr Watson did not find it so low in his route, but was par-

	Clova.	Braemar.	Fort William.	Tongue.
Upper limit of Myrica gale,	......	1350	1120	......
Pteris aquilina,	1600	......	1200	......
Erica cinerea,	2400	2200	2100	1730
Calluna vulgaris,	......	2900	2620	2440
Empetrum nigrum,	......	4100	3500	......
Lower limit of Carex rigida,	2325	2300	2070	1550
Azalea procumbens,	2350	2200	2025	1500

The highest stations of—	Clova.	Braemar.	Fort William.	Tongue.
Ulex europæus,	1550	1350	280	350
Lonicera Periclymenum,	1580	......	...	700
Corylus Avellana,	1600	1400	730	...
Cytisus Scoparius,	1700	1900	...	...

Taking Braemar as the standard, it is found, from the average of the first table, that a similar climate in Clova is 125 feet above this; while at Fort William it is 269, and at Tongue 595 feet below it. The difference is greater at lower elevations, so that vegetation, both natural and cultivated, is very little better near the sea-level at Tongue than we find it at a thousand feet above this on the banks of the Dee. The influence of situation is well exemplified by the fact that *Empetrum nigrum*, under the steep snow rocks on the northern side of Ben Nevis, fails 600 feet below its height on the western side; and *Calluna vulgaris*, on the northern precipitous slope of Cairn Garidh, ceases 400 feet below its limit on the western declivity of the same mountain. In our ascent, we find the following plants about 1500 feet below *Carex rigida:*

Saxifraga aizoides.	Tofieldia palustris.
Alchemilla alpina.	Epilobium alsinifolium.
Oxyria reniformis.	Carex capillaris.

About 500 feet higher up, we begin to see

Luzula spicata.	Silene acaulis.
Thalictrum alpinum.	Potentilla alpestris.
Saxifraga oppositifolia.	Dryas octopetala.
Rhodiola rosea.	

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ticularly struck with the rapid descent of alpine vegetation as he went west. He found at the sea level, on limestone cliffs at Loch Erriboll, *Carex capillaris*, *Draba incana*, *Saxifraga oppositifolia*, and *Dryas octopetala*; and the three last equally low at Farr, but they were not seen at Tongue. The neighbourhood of the north shore enables *Pinguicula lusitanica*, generally confined to the west coast, to grow a considerable way to the eastward. I formerly gathered it on Ben Hope, and Mr Watson found it still farther east, at *Strathey*.—*R.* G.)

A little higher we have

Rubus Chamæmorus.
Betula nana.
Epilobium alpinum.
Cornus Suecica.
Arbutus alpina.
Draba incana.

Bordering closely on the confines of *Carex rigida*, are

Juncus trifidus.
Arabis petræa.
Gnaphalium supinum.
Hieracium alpinum.

Above the commencement of *Carex rigida*, we first see

Sibbaldia procumbens.
Cerastium alpinum.
Aira alpina.
Poa alpina.
Salix herbacea.
Astragalus alpinus.

Here, too, is the natural climate of some species occasionally carried lower by streams or debris from rocks, viz.

Veronica alpina.
.... saxatilis.
Carex atrata.
.... Vahlii.
Alopecurus alpinus.
Phleum alpinum.
Sonchus alpinus ? *
Saxifraga nivalis.
Cherleria sedoides.
Spergula saginoides.

Stellaria cerastoides and *Saxifraga rivularis* are scarcely seen below the upper limit of *Calluna vulgaris;* and *Luzula arcuata* on Ben-na-muic-dui only commences a thousand feet above this.

With respect to the upper limits of species. On the small space of Ben-na-muic-dui rising above *Empetrum nigrum* were only observed

Luzula spicata.
.... arcuata.
Silene acaulis.
Salix herbacea.
Carex rigida.
Festuca vivipara ?
Lycopodium Selago.

These constitute the vegetation of the summit ; and, very nearly as high, were

Vaccinium Myrtillus.
Aira alpina.
Gnaphalium supinum.
Juncus trifidus.
Viola palustris.

Excepting *Luzula arcuata*, all these were seen above *Empetrum nigrum* on the Ben Nevis range (in which Cairn Garidh is included), and, along with them, several others, which perhaps may exist equally high on Ben-na-muic-dui, viz.

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* Mr Watson marks this with doubt, from being necessarily ignorant of the elevations of the new stations at which *Sonchus alpinus* was found. Neither can I speak positively, but my impression is as he states it. All the new stations are, like the old one, in deep shaded ravines, with a northern aspect.—R. G.

Saxifraga stellaris.
.... rivularis.
Sibbaldia procumbens.
Rumex acetosa.
Leontodon —? (not in flower).
Alchemilla alpina.
.... vulgaris.
Galium saxatile.
Cochlearia officinalis?
Stellaria cerastoides.
.... uliginosa.
Epilobium alpinum.
Rhodiola rosea.
Cerastium latifolium.
Ranunculus acris.
Veronica humifusa.
.... alpina.
Euphrasia officinalis.
Statice Armeria.
Poa alpina.
Oxyria reniformis.
Silene maritima.
Aira flexuosa.
Thymus Serpyllum.
Cryptogramma crispa.
Polypodium Phegopteris.
Thalictrum alpinum.
Chrysosplenium oppositifolium.
Cerastium latifolium.
.... viscosum.
Polygonum viviparum.
Carex pulla.
Trollius europæus.

Nearly at the same altitude as *Empetrum nigrum*, terminate

Lycopodium alpinum.
Blechnum boreale.
Vaccinium uliginosum.
Caltha palustris.

Descending towards the line of *Calluna vulgaris*, we meet with

Pinguicula vulgaris.
Campanula rotundifolia.
Eriophorum angustifolium.
Eleocharis cæspitosa.
Rubus Chamæmorus.
Juncus squarrosus.
Luzula campestris.
.... sylvatica.
Scabiosa succisa.
Oxalis Acetosella.
Narthecium ossifragum.
Solidago virgaurea *.
Arbutus alpina.
Azalea procumbens.
Arabis petræa.
Anthoxanthum odoratum.
Tormentilla officinalis.
Carex pilulifera.
.... pulicaris.
Lycopodium selaginoides.
Vaccinium Vitis-idæa.
Nardus stricta.
Achillæa Millefolium *.
Saxifraga hypnoides.

Such constitute the most alpine vegetation of Britain. On descending from the upper limits of *Calluna vulgaris*, the accessions become too numerous to detail: a few may be noticed. *Betula alba* I saw a seedling of among the rocks of Ben Nevis, 2700 feet high, *there* almost the upper limit of *Empetrum nigrum*, and equal to 3500 on a better aspect. Between the upper line of *Calluna vulgaris*, and lower limit of *Carex rigida*, is the greatest height reached by

Pinus sylvestris.
Pyrus aucuparia.
Juniperus communis.
Arbutus Uva-ursi.

It is only below *Carex rigida* by 1000 feet or more, that we find the upper limits of the oak, ash, beech, sycamore, holly, cherry, and hawthorn. The roses and shrubby brambles (except *Rubus idæus*) are almost equally distant; and with them is found the upper limit of agriculture. At Clova, *Ulex europæus* exceeds cultivation by six or seven hundred feet; in the other three stations it is rather below. In none of them does the climate admit the successful cultivation of wheat: Braemar is too high; Fort William is too wet; Glen Clova, a narrow valley shaded from the sun by high hills; and Tongue, exposed to a north sea, and with high ground to the south:—all therefore are equally unsuited. At Clova it has been tried, but failed to ripen.

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* I have seen both these plants on dry broken quartz, within a few feet of the summit of Ben More, Assynt, dwindled down to one or two inches in height, but in full flower; and Mr Watson observed the same thing on Ben *Hope.*—R. G.

Description of several New or Rare Plants which have lately flowered in the neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany in the University of Edinburgh.

Sept. 10. 1832.

Banksia media.

B. *media;* foliis cuneato-linearibus truncatis dentato-serratis basi attenuatis: subter reticulatis venis venulisque glabratis lacunis tomentosis, perianthii unguibus sericeis; laminis glabris, folliculis glabriusculis immersis floribus marcescentibus.—*Br.*

Banksia media, *Br.* Prodr. Fl. Nov. Holland. Suppl. 1. p. 35.

DESCRIPTION.—*Shrub* erect. *Branches* subverticillate and spreading wide slightly ascending at the extremities, grey with short dense tomentum. *Leaves* (3–7 inches long, 6–10 lines broad near the apex), scattered, petioled, spreading moderately, linear-spathulate, serrate, truncate, above glabrous and shining, below reticulated, pale, pitted, and the pits filled with white tomentum. *Amentum* (3 inches long, 2¼ broad) terminal, cylindrical, its scales covered with red-brown tomentum; the lower flowers expanding a little before the upper. *Perianth* 4-parted, segments linear, narrow; claws covered with yellow tomentum on the outside, glabrous within; limb green, streaked with brown, at first hairy, afterwards subglabrous.

This species is placed by Mr Brown immediately after *B. marcescens.* It promises to be a very handsome addition to the species in cultivation, and is now flowering most freely in the greenhouse of the Botanic Garden, Edinburgh, to which establishment it was obligingly communicated from the Clapton nursery. The first amentum expanded its flowers in the beginning of September, but as the whole plant is covered with others in all stages, they will probably expand in succession for a very long while.

Euphorbia cruentata.

E. *cruentata;* caule herbaceo, ramoso, erecto, foliisque oppositis pedunculatis lanceolatis maculatis inequaliter serratis piloso; bracteis lineari-lanceolatis; cymis terminalibus subtrifidis, decompositis; involucro fimbriato; appendice unico cyathiforme integro; fructu glabro; seminibus verrucosis.

Description.—*Stem* herbaceous, erect, branched, hairy, particularly towards its extremity. *Leaves* (2 inches long, 10 lines broad) petioled, lanceolate, unequally serrated, above irregularly sprinkled with dull red spots, hairy, particularly below, where they are much paler without the red spots, and with very prominent middle rib and veins. *Bracteæ* linear-lanceolate, more entire, but in other respects resembling the leaves. *Cymes* terminal, subtrifid, crowded; primary rays trifid, and their subdivisions irregular. *Involucre* fimbriated, provided with only one cup-shaped entire appendage. *Male flowers* on pedicels longer than the filament, anthers yellow, lobes bursting along the vertex. *Germen* green, glabrous, but not shining; styles revolute, white. *Fruit* smooth. *Seeds* brown, verrucose.

Seeds of this plant were sent along with specimens to this country from St Louis, North America, by Mr Drummond. The plants flowered in the greenhouse of the Botanic Garden in August and September. The only difference between the cultivated and wild specimens arises from the greater vigour of the former: they are larger, and the leaves are nearly rhomboid.

Œnothera perampla.

O. *perampla;* caule ramoso, tenuissime rubescenti, foliis runcinatis, pubescentibus, lobo terminali maximo acuminato undulato basi sublobato; floribus axillaribus, omnibus diffusis; calycis limbo petala subrotundata integerrima æquante, tubo longissimo; capsulis muticis tetrapteris, alis deorsim truncatis.

Description.—*Root* perennial. *Stem* herbaceous, robust and much branched, covered with very fine pubescence, green; branches red only at their origins, ascending. *Leaves* alternate, petioled, minutely pubescent on both sides, undulate, dentate, runcinate, pale and with prominent veins below, terminal lobe very large acuminate incised at its base, lower lobes much smaller linear-lanceolate acute spreading. *Flowers* solitary, axillary, very large and handsome, sessile. *Calyx* pale green, minutely pubescent, nearly as long as the leaf; tube (4½ inches long) erect, red on base when young; limb (2 inches long) acuminate. *Corolla* white, with a yellowish tinge; petals subrotund, entire, equal in length to the calyx, 3-ribbed and veined, the lateral ribs furnishing secondary ribs from the outside of their base. *Stamens* included, declined; filaments tumid at their insertion into the throat of the calyx, subulate, pale yellow in their lower half, white above. *Anthers* (three-fourths of an inch long) linear, nearly entire at both extremities, versatile, bursting long before the buds expand; pollen granules triangular. *Stigma* 4-parted, lobes spreading. *Style* as long as the stamens, flattened, linear. *Germen* oblong, covered with the same pubescence as on other parts of the plant, 4-winged in its upper half, wings narrower upwards, truncated below. *Ovules* very numerous, and closely imbricated.

The seeds of this very fine species were received from my friend Mr Cruckshanks, and were probably gathered somewhere in the neighbourhood of Lima, but at a considerable elevation, for the plant is quite hardy, and has flowered in the open air at the Royal Botanic Garden during the last two seasons. It is very deserving of cultivation. It most nearly resembles *O. taraxacifolia*, but is distinguished from this by its less prostrate growth; by the green colour of its stem; by the capsule being more attenuated at its base; by the absence of the conspicuous glands and hairs, which are mixed in *O. taraxacifolia* with the finer pubescence on the capsule and stem; and by the tinge of yellow, not of purple, in the flower. Its foliage more nearly resembles that of *O. triloba,* but it is distinguished from this by its much greater size, by its nearly white flowers, and by its much darker foliage. From both these species it is farther distinguished, by the flowers being altogether diffused over the branches, and never crowded near the root, as so many of them are in *O. taraxacifolia* and *O. triloba;* and, lastly, by the absence of the lobed crown to the capsule which both of these have.

Physianthus albens.

P. *albens;* herbacea, volubilis, foliis oppositis integerrimis acutis basi cordato-truncatis subtus albo-pruinosi, floribus subdichotomo-cymosis. —*Martius.*

Physianthus albens, *Mart.* Nov. Genera et Sp. Plant. Brasil. i. 54. t. 32.

Description.—*Root* woody, branched, and fibrous. *Stem* woody (at least when cultivated in the stove), round, branched, twining; bark green, cracked, and on the recent shoots, which are very long and slender, pretty densely covered with short adpressed pubescence. *Branches* opposite and axillary, spreading. *Leaves* (3 inches long, 1¾ broad) petioled, opposite, oblong, truncated below, undulate, entire, acute, deep green and pruinose above, paler below, and there especially clothed with minute pubescence. *Petiole* about one-third of the length of the leaf, of the same colour with the shoots, channelled above, spreading. *Peduncles* lateral, more rarely axillary, subdichotomously cymose, 4–8-flowered, about as long as the petiole, and like it; pedicels (about 7 lines long) spreading, straight. *Calyx* 5-parted, green, very minutely tomentous, obscurely veined; segments ovate, acute, spreading below, erect in their upper half, reflected at the sides. *Corolla* faintly perfumed, somewhat fleshy, white, when in bud pale rose-coloured, hypocrateriform, glabrous; tube (half an inch long) one and a half times as long as the calyx, at its base ventricose with five gibbosities and slightly hairy on the inside, above 5-gonous, sides depressed, and having a ridge in the centre of the depression; limb (1¼ inch across) spreading, 5-parted, segments ovate, acute, reflected at the apices and at the sides. *Crown* attached to the inside of the base of the tube, 5-parted, lobes connivent, blunt, convex on the outside, alternate with the gibbosities of the tube, glabrous. *Stamens* opposite to the lobes of the crown, and twice as long as these, adpressed to the pistil; filaments coarse and fleshy, monadelphous, concave on the inside, flat on the out, sagittate above, terminated by a little ovate subacute point, below the sides of which, and on the inside of the filament, are the cells of the anther; pollen-masses yellow, elliptico-ovate, flattened, reticulated. *Stigma* large, conical, angular, terminated above by two appendages longer than itself, which diverge below, meet above near the apices, and again diverge; glands alternate with the stamens, indented into the angles of the stigma, deep lilac, cartilaginous, slit vertically along their outer surface, terminated above by a cordate brown process, emarginate at the apex, and below by two processes, which are brown, linear, flat, swollen at both their extremities, each becoming attached obliquely to the narrower extremity of a pollen-mass in the stamen next to it. *Styles* 2, short, connivent above. *Germens* 2, turgid, ovate, acute. *Ovules* very numerous, small, imbricated, filamentous, attached to the receptacle placed on the inside of the germen.

Seeds of this fine plant were received by Mr Neill from Mr Tweedie, Buenos Ayres, in 1830, and, climbing along the roof of the stove in his garden, flowered freely in August last. I possess from Mr Tweedie an excellent specimen, in no respect different from the cultivated plant.

Stylidium junceum.

S. *junceum;* foliis radicalibus linearibus: scapi stricti glabri minutis distantibus bracteisque medio adnatis, calycis laciniis subulatis: basi simplici, fauce glandulis stipitatis coronata, lobello inappendiculato.—*Br.*

Stylidium junceum, *Brown's* Prodr. Fl. Nov. Holland. 1. 569.

Description.—*Root* fibrous, perennial, pushing several stems (scapes) from the crown. *Root-leaves* crowded, linear, glabrous, somewhat fleshy, callous at the tip: *stem-leaves* (bracteæ) minute, green and fleshy, attached by the middle, acuminated at both ends, or rarely emarginate below, adpressed. *Scapes* (nearly 2 feet high) erect, slender, green, round, glabrous. *Raceme* terminal, lax; pedicels resembling the extremity of the scape, solitary from the axils of the bracteæ, and frequently bracteolate, pubescent on their upper side, pubescence glandular. *Calyx* segments subulate, unequal, glabrous. *Corolla* small, rose-coloured, tube longer than the calyx, twisted; faux very oblique, crowned with a few glandular hairs; limb glabrous on the inside, sparingly covered with glandular pubescence on the out, segments ovate, blunt; labellum oblongo-ovate, inappendiculate. *Germen* green, turbinate, with five gibbosities at the top from which the calyx-segments spring. *Column* flat, much longer than the corolla, tapering below and above broadest where first deflected over the labellum, and at this point lilac, below white, above rose-coloured, everywhere glabrous, dilated fleshy and reticulated at the summit. *Anthers* small, yellow, glabrous. *Stigma* prominent, green, pubescent.

This plant, less ornamental than perhaps any of the species hitherto introduced into cultivation, but still interesting, was raised, from seed from King George's Sound, at the Caledonian Horticultural Society's Garden, last year. One of the seedlings, communicated to Mr Neill, Canonmills, came into flower in the beginning of September 1832, in consequence of the judicious treatment it received. We have raised it at the Botanic Garden this season, and many of the plants are pushing up their scapes, but the flowers will not be expanded for some time.

Tropæolum pentaphyllum.

T. *pentaphyllum;* foliis digitato-quinatis; foliolis ovalibus, integerrimis, petiolatis; petalis duobus, subrotundis, subsessilibus, calyce multo brevioribus; calcare recto, apice ovato carnoso ascendenti.

Tropæolum pentaphyllum, *Lam.* Encycl. Method. 1. 612. pl. 277. fig. 2. —*Willd.* Spec. Pl. 2. 299.—*Pers.* Synops. 1. 405.—*De Cand.* Prodr. 1. 684.—*Spreng.* Syst. Veget. 2. 226.

Description.—*Root* tuberous, large, oblong. *Stem* slender, greatly elongated, slightly twisted, round, glabrous, coloured, branched. *Leaves* (about 2 inches across) petioled, digitate, of 5 oblong entire petiolate soft glabrous spreading leaflets. *Common petiole* (2 inches long) twisted in form of a tendril, and forming the chief support of the stem, as well as the partial petioles and the veins of the leaf purple and glabrous: *partial petioles* bordered by the decurrent leaflets. *Peduncles* (4 inches long) solitary, axillary, longer than the leaves, purple, glabrous, thickening upwards, pendulous. *Calyx* (1¼ inch long) persisting; spur horizontal, fleshy, dull purple on the outside, yellow within, nectariferous, conical, till towards its apex, when it is contracted, thinner, and somewhat shrivelled, the apex being ovato-acute, fleshy and erect; limb (7½ lines across) 5-parted, green, brighter and spotted or streaked with deep purple within, segments ovato-acute, the uppermost the narrowest, the two next to it the broadest. *Petals* 2, small, subrotund, subunguiculate, reflected, bright vermilion-coloured, inserted into the throat of the calyx on each side of the upper segment. *Stamens* 8, longer than the calyx segments; filaments subulate, declined, closely streaked or spotted with purple, in the bud erect, turned out between the calyx segments after the pollen is shed; anthers four-sided, oblong, truncated above and below, green; pollen green. *Germen* yellow, glabrous. *Style* yellow, 3-sided, shorter than the stamens. *Stigmata* 3, acute, diverging. *Fruit* 3-coccous, glabrous, even.

Mr Neill received at his garden at Canonmills a tuber gathered by Mr Tweedie in 1829. It pushed out some feeble shoots in 1830, and also last year, and is now growing most vigorously, settling a question of which De Candolle was doubtful,—that the species is perennial. A cutting taken from it, flowered in the greenhouse for the first time, and most freely, during June and July 1832, and will probably ripen its seeds. From Mr Tweedie I have excellent native specimens, gathered *in hedges* near Buenos Ayres. Its taste is very similar to that of *Tropæolum majus,* but less pungent, and less agreeable.

Celestial Phenomena from October 1. 1832 *to January* 1. 1833, *calculated for the Meridian of Edinburgh, Mean Time.* By Mr George Innes, Astronomical Calculator, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight —The Conjunctions of the Moon with the Stars are given in *Right Ascension.*

OCTOBER.

D.	H.	′	″		D.	H.	′	″	
1.	19	14	6	☽ First Quarter.	16.	19	17	13	Em. II. sat. ♃
1.	21	33	4	Em. I. sat. ♃	16.	20	6	18	☾ Last Quarter.
2.	3	34	12	☌ ☽ σ ♐	17.	17	50	4	☌ ☽ δ ♋
2.	6	0	22	☌ ☽ π ♐	17.	19	52	48	Em. I. sat. ♃
2.	16	48	36	☌ ☿ β ♍	19.	22	41	18	Im. III. sat. ♃
2.	19	48	41	Im. IV. sat. ♃	20.	1	43	9	Em. III. sat. ♃
2.	22	45	1	Em. IV. sat. ♃	21.	0	7	21	☌ ☽ ♄
4.	13	49	17	☌ ☽ ϑ ♑	23.	8	28	–	Sup. ☌ ☉ ☿
4.	20	14	51	☌ ☽ ♅	23.	9	26	35	☉ enters ♏
5.	6	35	57	☌ ☽ γ ♑	23.	15	17	9	☌ ☽ ☿
5.	10	2	37	☌ ☽ δ ♑	23.	18	20	18	● New Moon.
7.	6	47	41	☌ ☿ η ♍	23.	21	52	45	Em. II. sat. ♃
7.	17	49	17	☌ ☽ ♃	24.	21	48	26	Em. I. sat. ♃
8.	9	32	8	☌ ♀ λ ♍	25.	3	46	13	☌ ♀ κ ♎
8.	23	28	26	Em. I. sat. ♃	25.	10	52	14	☌ ☽ γ ♎
9.	19	10	48	○ Full Moon.	25.	14	41	16	☌ ☽ ♀
10.	7	5	13	☌ ☽ ν ♓	25.	19	21	22	☌ ☽ ϑ ♎
10.	17	57	16	Em. I. sat. ♃	27.	9	3	44	☌ ♀ λ ♎
11.	5	14	8	☌ ☽ 2 ξ Ceti.	27.	11	37	3	☌ ☽ ϱ Oph.
11.	12	57	38	☌ ☽ μ Ceti.	28.	10	41	23	☌ ♂ 2 κ ♉
12.	21	41	50	Em. III. sat. ♃	28.	11	1	58	☌ ☽ 1 μ ♐
13.	8	7	58	☌ ☽ 1 δ ♉	28.	12	43	7	☌ ☽ 2 μ ♐
13.	8	37	40	☌ ☽ 2 δ ♉	29.	11	51	34	☌ ☽ σ ♐
13.	12	13	34	☌ ☽ ♂	29.	14	6	39	☌ ☽ π ♐
14.	14	58	48	☌ ☽ ζ ♉	30.	2	27	12	☌ ♄ β ♍
14.	23	45	59	☌ ♀ 2 α ♎	31.	0	28	20	Em. II. sat. ♃
15.	23	36	7	☌ ☽ ν ♊	31.	15	36	48	☽ First Quarter.
16.	1	23	56	Em. I. sat. ♃	31.	21	51	0	☌ ☽ ϑ ♑
16.	1	33	9	☌ ☽ ζ ♊	31.	23	44	11	Em. I. sat. ♃
16.	7	52	1	☌ ☽ δ ♊					

NOVEMBER.

D.	H.	′	″		D.	H.	′	″	
1.	3	6	–	☍ ☉ ⚳	7.	21	22	44	☌ ☽ μ Ceti.
1.	4	28	26	☌ ☽ ♅	8.	8	1	0	○ Full Moon.
1.	14	43	58	☌ ☽ γ ♑	8.	13	46	35	☌ ☿ κ ♎
1.	18	12	12	☌ ☽ δ ♑	9.	10	29	13	☌ ☽ ♂
2.	18	13	10	Em. I. sat. ♃	9.	15	36	41	☌ ☽ 1 δ ♉
3.	22	34	42	☌ ☽ ♃	9.	16	5	38	☌ ☽ 2 δ ♉
6.	15	51	28	☌ ☽ ν ♓	9.	20	9	2	Em. I. sat. ♃
7.	13	46	21	☌ ☽ 2 ξ Ceti.	10.	10	31	16	☌ ☿ λ ♎

NOVEMBER—*continued.*

D.	H.	′	″	
10.	21	38	1	☌ ☽ ζ ♉
11.	10	50	48	☌ ☿ δ ♏
11.	15	19	16	☌ ☽ μ ♊
11.	17	43	36	☌ ☽ ν ♊
12.	7	21	51	☌ ☽ ζ ♊
12.	13	33	8	☌ ☽ δ ♊
13.	5	47	13	☌ ♀ ϑ Oph.
13.	15	27	22	☌ ♂ 1 A ♉
13.	23	5	9	☌ ☽ δ ♋
14.	1	43	29	☌ ♀ B Oph.
15.	4	23	36	☾ Last Quarter.
16.	22	4	57	Em. I. sat. ♃
17.	10	43	41	☌ ☽ ♄
17.	17	50	52	Em. III. sat. ♃
17.	18	57	25	Em. II. sat. ♃
20.	14	4	27	☍ ☉ ♂
22.	5	57	41	☉ enters ♐
22.	8	50	48	● New Moon.
23.	7	24	19	☌ ☿ A Oph.
23.	18	44	16	☌ ☽ ☿
23.	19	54	36	☌ ☽ ρ Oph.
24.	7	53	48	☌ ☿ ϑ Oph.
24.	18	55	43	Im. III. sat. ♃
24.	20	13	14	☌ ☽ 1 μ ♐
24.	20	53	10	☌ ☽ 2 μ ♐
24.	21	7	12	☌ ☿ B Oph.
24.	21	33	6	Em. II. sat. ♃
24.	21	52	58	Em. III. sat. ♃
25.	1	53	8	☌ ☽ ♀
25.	10	11	38	☌ ♀ λ ♐
25.	18	29	53	Em. I. sat. ♃
25.	19	42	14	☌ ☽ ο ♐
25.	22	6	19	☌ ☽ π ♐
28.	5	40	32	☌ ☽ ϑ ♑
28.	12	18	52	☌ ☽ ♅
28.	22	38	0	☌ ☽ γ ♑
29.	2	7	43	☌ ☽ δ ♑
30.	12	34	6	☽ First Quarter.

DECEMBER.

D.	H.	′	″	
1.	7	48	24	☌ ☽ ♃
1.	22	58	55	Im. III. sat. ♃
2.	20	25	53	Em. I. sat. ♃
4.	1	41	16	☌ ☽ ν ♓
4.	3	39	26	☌ ♀ ψ ♐
4.	23	50	11	☌ ☽ 2 ξ Ceti.
5.	7	28	57	☌ ☽ μ Ceti.
5.	9	52	16	☌ ☿ λ ♐
6.	3	52	13	☌ ☽ ♂
7.	1	30	2	☌ ☽ 1 δ ♉
7.	1	58	35	☌ ☽ 2 δ ♉
7.	10	15	–	☿ greatest elong.
7.	20	6	30	○ Full Moon.
8.	6	54	35	☌ ☽ ζ ♉
8.	21	3	25	☌ ☽ η ♊
8.	21	23	2	Im. IV. sat. ♃
9.	0	5	38	☌ ☽ μ ♊
9.	2	25	22	☌ ☽ ν ♊
9.	15	36	57	☌ ☽ ζ ♊
9.	21	35	41	☌ ☽ δ ♊
9.	22	21	54	Em. I. sat. ♃
11.	5	59	13	☌ ☽ δ ♋
11.	16	50	56	Em. I. sat. ♃
14.	12	13	54	☾ Last Quarter.
14.	19	5	27	☌ ☽ ♄
17.	*22*	*38*	*16*	☌ ☿ 1 ν ♐
18.	*18*	*46*	*59*	Em. I. sat. ♃
19.	1	29	51	☌ ☽ γ ♎
19.	10	11	33	☌ ☽ ϑ ♎
19.	18	38	21	Em. II. sat. ♃
21.	3	0	34	☌ ☽ ρ Oph.
21.	18	39	4	☉ enters ♑
22.	2	22	12	● New Moon.
22.	14	32	17	☌ ☽ ☿
22.	22	3	58	☌ ☽ 1 ν ♐
23.	2	58	10	☌ ☽ ο ♐
23.	5	22	13	☌ ☽ π ♐
24.	23	15	43	Inf. ☌ ☉ ☿
25.	12	30	34	☽ very near ♀
25.	12	43	52	☌ ☽ ϑ ♑
25.	14	51	44	☌ ♀ ϑ ♑
25.	17	34	32	Em IV. sat. ♃
25.	20	42	58	Em. I. sat. ♃
25.	22	8	33	☌ ☽ ♅
26.	5	26	20	☌ ☽ γ ♑
26.	9	16	26	☌ ☽ δ ♑
26.	12	3	22	☌ ☿ 2 μ ♐
26.	19	8	41	☌ ☿ 1 μ ♐
26.	21	14	16	Em. II. sat. ♃
28.	20	42	41	☌ ☽ ♃
29.	15	21	8	☌ ♀ ♅
30.	7	59	42	☽ First Quarter.
30.	18	2	47	Em. III. sat. ♃
31.	10	54	55	☌ ☽ ν ♓

Times of the Planets passing the Meridian, and their Declinations.

OCTOBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	10 55	5 9 N.	12 57	9 30 S.	3 39	20 4 N.	22 50	4 45 S.	10 49	5 22 N.	20 28	17 5 S.
5	11 4	2 49	13 0	11 24	3 25	20 18	22 32	4 57	10 35	5 12	20 12	17 6
10	11 15	1 13 S.	13 4	13 41	3 7	20 32	22 10	5 9	10 18	4 58	19 52	17 7
15	11 27	4 26	13 8	15 50	2 48	20 44	21 49	5 20	10 0	4 44	19 32	17 7
20	11 38	8 7	13 13	17 49	2 27	20 54	21 28	5 29	9 43	4 32	19 13	17 8
25	11 49	11 34	19 18	19 36	2 5	21 1	21 6	5 37	9 25	4 20	18 53	17 8

NOVEMBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	12 5	16 37 S.	13 27	21 42 S.	1 31	21 6 N.	20 38	5 44 S.	9 0	4 3 N.	18 25	17 7 S.
5	12 14	18 47	13 33	22 45	1 10	21 5	20 21	5 46	8 46	3 54	18 9	17 6
10	12 26	21 10	13 38	23 46	0 43	21 2	20 1	5 47	8 28	3 44	17 49	17 5
15	12 38	23 6	13 45	24 29	0 16	20 55	19 51	5 46	8 10	3 35	17 30	17 4
20	12 50	24 34	13 54	24 54	23 49	20 45	19 32	5 43	7 52	3 26	17 11	17 2
25	13 2	25 30	14 0	25 0	23 22	20 34	19 3	5 38	7 33	3 18	16 52	17 0

DECEMBER.

	Mercury.		Venus.		Mars.		Jupiter.		Saturn.		Georgian.	
D.	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′	H. ′	° ′
1	13 15	25 51 S.	14 9	24 43 S.	22 51	20 18 N.	18 40	5 29 S.	7 12	3 9 N.	16 29	16 57 S.
5	13 21	25 35	14 15	24 16	22 29	20 11	18 25	5 22	6 58	3 5	16 14	16 53
10	13 21	24 47	14 22	23 27	22 4	20 2	18 7	5 12	6 39	2 59	15 55	16 50
15	13 8	23 26	14 28	22 21	21 41	19 56	17 49	4 59	6 20	2 55	15 36	16 47
20	12 36	22 13	14 34	20 58	21 19	19 54	17 32	4 45	6 0	2 52	15 17	16 43
25	12 4	21 10	14 40	19 21	20 59	19 56	17 14	4 29	5 49	2 50	14 59	16 38

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Unctuous Dew.*—In the neighbourhood of Rotterdam, it has been recently observed, that the morning dews, instead of being pure and limpid, are of an unctuous consistency.

CHEMISTRY.

2. *The New Vacuum Sugar.*—The grains of this beautiful sugar are true and well-formed crystals. They do not melt so readily as common sugar,—a circumstance that induces some inaccurate observers to imagine that this sugar is not so sweet as common muscovada. The taste is just that of fine *candy*. The advantage is, that this sugar is far less hygrometric than common raw sugar, and suffers less from a moist atmosphere. The apparatus used in its preparation is a modification of the late Mr Howard's apparatus for boiling sugar *in vacuo;* with strainers of copper-plates pierced with minute holes, or several folds of wire-gauze for clarifying the syrup. The process is, immediately on crushing the canes, to heat, lime, and scum the juice, which, while warm, is forced through the strainer, from which it runs into the boilers. These are provided with air-tight covers, the tops of which are connected by tubes with a large air-pump, wrought by a steam engine. The steam, as generated, is thus drawn off, and the boiling is carried on at a temperature far below the boiling point of sugar. When sufficiently concentrated, the syrup is crystallized, and, when consolidated, it is carried to the curing-house, the temperature of which is kept up by steam-pipes running into it. This process saves much sugar, for the heating being low, little or no molasses are formed, and a large quantity of sugar is obtained, which, in the old process, is converted into molasses. This apparatus was adopted by sugar-growers in Demerara, on the suggestion of a distinguished philosopher in Liverpool. The experiment has succeeded admirably; and the sugar bears a premium in the Liverpool market, especially when required for coffee.

3. *On the Grease of Wines.*—White wine is subject to an alteration which is designated in Switzerland, and other countries, by the terms *greasy* and *ropy*, (tourner au gras, graisser, filer), a change which takes place after the vinous fermentation has apparently ceased, and the wine has been bottled or closely confined in casks. The wines of Champagne, of Switzerland, and most thin and light wines, are very subject to it, especially when the vintage has been wet. The cause of this malady resides in a mucilaginous principle which is developed in light wines: it pervades the whole mass, and puts on a reticulated appearance; a similar change is observable in beer, and in syrups made of sugar of an inferior quality. Various methods have been pursued for remedying this defect. Common salt is added to the wine, a practice which was adopted, it is said, by the Romans, in consequence of an accidental discovery of an amateur in wines. Having opened an amphora of wine, and being struck with its excellence, he demanded of his slave what he had put in it. The latter, mistaking his master's meaning, fell on his knees and confessed he had drank a little wine and filled up the vessel with sea-water. After two or three months, it is impossible for the most delicate palate to distinguish the taste of salt, and it is admitted that such an addition improves the taste of the wine, but that it prevents the *grease* is a point much more doubtful. Another remedy is the addition of brandy or alcohol. But the more efficacious means of all is a frequent racking off, or decantation. Wine must never be allowed to *whiten*, that is, to admit the rising of a milky substance, which destroys its transparency. When this disease has been contracted, it may often be removed by clarification with fish glue; but this remedy has two inconveniences,—it does not always succeed, and when it does, it diminishes the strength of the wine. This deterioration arises either from the glue, or perhaps from the disease itself, which has occasioned the operation. Another method of clarifying wines and removing the grease, consists in filtering them through shavings of hazel. For small quantities this method is very good. When the sale of wine is not pressing, and care is taken to keep the vases which contain them full, and they are allowed to undergo a slow and insensible fermentation, and are exposed to the change of temperature which the season brings round, this disease spon-

taneously disappears. It is rare that greased wines thus treated are not cured in passing through the cold of one winter. The attention of chemists has been much engaged with the nature of this quality in wine. M. François of Chalons-sur-Marne, ascribes it to a substance which is found also in the gluten of wheat flour, and which M. Taddei, an Italian chemist, discovered and named Gliadine. It is the portion which is soluble in alcohol, the insoluble portion he called Zimome. If an alcoholic solution of gliadine be added to clear wine, it becomes milky, and assumes, according to M. François, the aspect of greased wines. Berzelius, however, does not believe in the gliadine of Taddei. He considers it to be gelatine, and the zimome to be albumen, both of which have been long known to exist in the gluten. The same chemist has proved that vegetable and animal gelatine are identical in the properties of uniting with tannin and forming an insoluble precipitate. However this may be, M. François has been induced to regard tannin as a remedy for the grease of wine. He accordingly makes an observation which seems to have escaped all those who had previously examined the subject, that red wines are never subject to the grease. Now, the difference between red and white wine is, that the red always ferments in presence of the husk and seeds of the grape, substances which contain tannin in abundance, while white wine remains in contact with the husk but a very short time. It is also a fact, that light wines made of grapes deprived of their seeds are more subject to this disease than others. Hence it is probable, that the presence of tannin may, by precipitating the gelatine, prevent the phenomena of the grease. The following are Mr François's directions: By adding tannin to wine a month or six weeks prior to bottling, it may be preserved from the grease; and this substance being one of those which exist in wine, it may be added without fear, for it communicates no unnatural odour or taste. Twenty grains of tannin to a bottle of wine, or three and a half ounces to a hundred bottles previously well decanted from all sediment, is the proper dose, although in frequent cases this dose must be repeated. If any sediment remain in the wine, a much larger dose of tannin becomes necessary. M. François affirms that *this malady* in wine, when once destroyed, never returns. As *the tannin* of chemists is an expensive article, obtained from the gallnut by sulphuric acid, or by potash, it is probable that a substitute may be found in some of the astringent barks, or even in the seeds of the grape.

ZOOLOGY.

4. *Obesity.*—The celebrated *fat liver pies* of Strasburgh are made of the livers of geese, fattened with great attention. The animal is shut up in a cage, but little larger than its body, and is taken out but twice a-day, and then to be fed with about a quart of crude peas. They are introduced with a finger into the pharynx of the animal, which is thus made to swallow this enormous quantity of nourishment, and is then immediately shut up in its cage. The immediate result of this kind of life is a remarkable obesity, and an enormous development of the liver, which, without any notable change of structure, acquired a triple or quadruple enlargement of volume. Bibulous paper brought into close contact with this fat liver, immediately absorbs an oily matter, much like melted fat. These livers sometimes weigh eight or ten ounces, and sell at from three to five francs. The fattening of geese in this manner is a good speculation, for every part of the animal possesses an intrinsic value; the fat on many occasions is a substitute for butter, and the flesh is served at table, and although somewhat tough, is not the less nutritious; the feathers are much sought after, the quills serve for writing, and even the excrements sell at a high price as one of the richest of manures.

5. *Portable Milk.*—M. Dirchoff, the Russian chemist, who some time since discovered the process of making starch into sugar, has lately made several experiments upon milk: the result of which he has arrived at is curious. He is said to have found a mode of keeping milk for use for any definite space of time. The process of preserving is this: he causes new milk to be evaporated over a slow fire, until it is reduced to a powder. This powder is then put into a bottle, which is hermetically sealed. When the milk is wanted for use, it is only to dissolve some of the powder in a seasonable quantity of water, and the mixture so dissolved will have all the qualities, as well as the taste, of milk.—*Edinburgh Agricultural Journal.*

6. *Quantity of Eggs consumed in London.*—The eggs of hens are those most commonly used as food, and form an article

of very considerable importance in a commercial point of view. Vast quantities are brought from the country to London, and other great towns. Since the peace they have also been largely imported from the continent. At this moment, indeed, the trade in eggs forms a considerable branch of our commerce with France, and affords constant employment for a number of small vessels. It appears from official statements, that the eggs imported from France amount to about 60,000,000 a-year; and supposing them to cost, at an average, 4d. per dozen, it follows that the people of the metropolis and Brighton (for it is into them that almost all are imported), pay the French above L. 83,000 a-year for eggs; and supposing that the freight, importer's and retailer's profit, duty, &c. raise their price to the consumer to 10d. per dozen, their total cost will be L. 213,000. The duty in 1829 amounted to L. 22,189.—*MacCulloch's Commercial Dictionary.* [About fifteen years ago the number of eggs exported from Berwick-upon-Tweed to London amounted to L. 30,000 worth a-year.]—*Edinburgh Agricultural Journal.*

7. *Destruction of Fresh-water Fish by the admission of the Sea into a Lake.*—The following particulars of the phenomena attending the opening of Lake Lothing at Lowestoft to the Sea, where sea-borne vessels were first received into the new harbour at that place on the 3d of June last, may prove of interest in natural history. They are extracted from the East Anglian Newspaper of June 7. 1831. Some of the circumstances attending the junction of the salt and fresh waters in the first instance are remarkable. The salt-water entered the lake with a strong under current, the fresh-water running out at the same time to the sea upon the surface. The fresh-water of the lake was raised to the top by the eruption of the salt-water beneath, and an immense quantity of yeast-like scum rose to the surface of the lake. The entire body of the water in the lake was elevated above its former level; and, on putting a pole down, a strong under-current could be felt bearing it from the sea; and at a short distance from the loch, next the lake, there was a perceptible and clearly defined line where the salt-water and the fresh met, the former rushing under the latter, and upon this line salt-water might have been taken up in one hand and fresh *in the other.* The consequences of the admission of the briny waters have been fatal to thousands of the former inhabitants of

the peaceful lake. Its surface was thickly studded with the bodies of pike, carp, perch, bream, roach, and dace, multitudes of which were carried into the ocean, and thrown afterwards upon the beach, most of them having been bitten in two by the dog-fish, which abound in the bay. It is a singular fact that a pike of about 20 lb. weight was taken up dead near the Mutford end of the lake, and, on opening the stomach, a herring was found in it entire.—*Edinburgh Quarterly Journal of Agriculture*, No. xviii.

GEOLOGY.

8. *Elevations in Australia, New South Wales.*—The following elevations above the level of the ocean, of points on the road over the Blue Mountains to Bathurst, and interior to the westward of its meridian, were computed by John Oxley, Esq. late Surveyor-General, from differences of the column of mercury, taken simultaneously in Sidney, and at the respective stations, in the year 1817, which barometrical admeasurements have been, since that period, fully verified by others :—

Spring Wood (Military Post) 12½ miles from Emu Ford,	1297
Bridge over Ravine, 17 ditto ditto,	1814
Caley's Repulse, 18 ditto ditto,	2110
Christmas Swamp, 24 ditto ditto,	2466
King's Table Land, 26½ ditto ditto,	2798
Military post, 28 ditto ditto,	2820
32 miles,	3203
32½ miles,	3382
Blackheath, 41 ditto,	3411
Summit of New Pass,	3245
Base of ditto,	2714
Summit of Mount York, or Old Pass,	3309
Base of ditto,	2610
Vale of Clwyd, near Collitt's Inn,	2642
Depot at Cox's River, military post, (Lat. 33° 33′ S., Long. 150° 05′ E.)	2172
Mount Blaxland,	3074
Hill above Jock's Bridge,	3444
Fish River Bridge,	2669
Hill above ditto,	3254
Sidmouth Valley,	2606
Bathurst Flagstaff, (Lat. 33° 24′ S., Long. 149° 29′ 30″ E.)	2232
Depot of 1817, on the Lachlan River (Lat. 33° 40′ S., Long. 148° 20′ E.),	600
Field's Plains, on ditto (Lat. 33° 15′ S., Long. 147° 16′ E.),	500

9. *On Subterraneous and Ominous Sounds.*—In a former volume of the Journal, we communicated some curious details in regard to what have been called *subterranean and ominous sounds.* Sir John Herschell has lately considered this subject, and conjectures that the noises of Nakoos, in Arabia, may be owing to a subterraneous production of steam, by the generation and condensation of which, under certain circumstances, sounds are well known to be produced. They belong to the same class of phenomena as the combustion of a jet of hydrogen gas in glass tubes. He also remarks, that wherever extensive subterraneous caverns exist, communicating with each other, or with the atmosphere, by means of small orifices, considerable differences of temperature may occasion currents of air to pass through those apertures with sufficient velocity for producing sonorous vibrations. The sounds described by Humboldt, as heard at sunrise, by those who sleep on certain granitic rocks, on the banks of the Orinoco, may be explained on this principle. The sounds produced at sunrise, by the statue of Memnon, and the twang, like the breaking of a string, heard by the French naturalists to proceed from a granite mountain at Carnac, are viewed by him as referable to a different cause, viz. to pyrometric expansions and contractions of the heterogeneous material of which the statue and mountain consist. Similar sounds, and from the same cause, are emitted when heat is applied to any connected mass of machinery; and the snapping often heard in the bars of a grate affords a familiar example of this phenomenon. The following amusing account of an ominous sound is given by Gairdner in his book on the "Music of Nature:"—In one of the baronial castles of the north, which has been uninhabited for years, there were heard at times such extraordinary noises, as to confirm the opinion among the country people that the place was haunted. In the western tower an old couple were permitted to live, who had been in the service of the former lord, but so imbued were they with the superstitions of the country, that they never went to bed without expecting to hear the cries of the disturbed spirits of the mansion. An old story was current, that an heir-apparent had been murdered by an uncle, that he might possess the estate, who, however, after enjoying it for a time, was so annoyed by the sounds in the castle, that he retired with an uneasy conscience from the domain, and died in

France. Not many years ago, the property descended to a branch of the female line (one of the heroes of Waterloo), who, nothing daunted, was determined to make this castle his place of residence. As the noises were a subject of real terror to his tenantry, he formed the resolution of sleeping in the castle on the night he took possession, in order to do away these superstitious fears. Not a habitable room could be found, except the one occupied by the old gardener and his wife in the western turret, and he ordered his camp-bed to be set up in that apartment. It was in the autumn, at nightfall, that he repaired to the gloomy abode, leaving his servant, to his no small comfort, at the village inn; and after having found every thing comfortably provided, turned the large old rusty key upon the antiquated pair, who took leave of him to lodge at a farm hard by. It was one of those nights which are checkered with occasional gleams of moonshine and darkness, when the clouds are riding in a high wind. He slept well for the two first hours; he was then wakened by a low mournful sound that ran through the apartments. This warned him to be up and accoutred. He descended the turret stairs with a brilliant light, which, on coming to the ground floor, cast a gigantic shadow of himself upon the high embattled walls. Here he stood and listened, when presently a hollow moan ran through the long corridor, and died away. This was followed by one of a higher key, a sort of scream, which directed his footsteps with more certainty to the spot. Pursuing the sounds, he found himself in the hall of his ancestors, and vaulting upon the large oaken table, set down his lamp, and folding his cloak about him, determined to wait for the appearance of all that was terrible. The night, which had been stormy, became suddenly still; the dark flitting clouds had sunk below the horizon, and the moon insinuated her silvery light through the chinks of the mouldering pile. As our hero had spent the morning in the chase, Morpheus came unbidden, and he fell asleep upon the table. His dream was short; for close upon him issued forth the horrid groan; amazed, he started up and sprang at the unseen voice, fixing, with a powerful blow, his Toledo steel in the arras. The blade was fast, and held him to the spot. At this moment the moon shot a ray that illumined the hall, and showed that behind the waving folds there lay the cause concealed. His sword he left, and to the turret retraced his steps. When morning came, a welcome crowd greeting, asked if he had met the ghost? "O, yes!" replied the knight, "dead as a door nail behind the screen he lies, where my sword has pinned him fast; bring the wrenching bar and we'll haul the disturber out." With such a leader, and broad day to boot, the valiant throng tore down the screen where the sword was fixed, when lo! in a recess, lay the fragments of a chapel organ, and the square wooden trunks made for hallowed sounds were used as props to stay the work when the hall was coated round with oak. The wondering clowns now laughed aloud at the mysterious voice. It was the northern blast that found its way through the crannies of the wall to the groaning pipes, that alarmed the country round for a century.

10. *Fossil Frogs, &c.*—Goldfuss, in the Nova Acta Physico-Medica Acad. Cæs. Leop. Carol. Nat. Cur. for 1831, describes the following fossil remains of various amphibious animals he detected in brown coal, in the vicinity of Bern. 1. *Rana diluviana.* 2. *Salamandra oxygia*, two inches and a half long. 3. *Triton noachicus*, two inches in length. 4. *Ophis dubius.* This remarkable remain he is of opinion belongs either to a snake or a snake-shaped fish.

11. *On the Permanence of the Earth's Axis of Rotation.*—"It appears," says Mrs Somerville, in her admirable work, "from the marine shells found on the tops of the highest mountains, and in almost every part of the globe, that immense continents have been elevated above the ocean, which (ocean) must have engulfed others. Such a catastrophe would be occasioned by a variation in the position of the axis of rotation on the surface of the earth; for the seas tending to the new equator would leave some portions of the globe and overwhelm others. But theory proves that neither rotation, precession, nor any of the disturbing forces which affect the system, have the smallest influence on the axis of rotation, which maintains a permanent position on the surface, if the earth be not disturbed in its rotation by some foreign cause, as the collision of a comet, which may have happened in the immensity of time. Then, indeed, the equilibrium could only have been restored by the rushing of the seas to the new equator, which they would continue to do *till their* surface was every where perpendicular to the direction

of gravity. But it is probable that such an accumulation of the waters would not be sufficient to restore equilibrium, if the derangement had been great; for the mean density of the sea is only about a fifth part of that of the earth; and the mean depth, even of the Pacific Ocean, is not more than four miles, whereas the equatorial radius of the earth exceeds the polar radius by twenty-five or thirty miles; consequently, the influence of the sea on the direction of gravity, is very small; and, as it thus appears that a great change on the position of the axis is incompatible with the law of equilibrium, the geological phenomena must be ascribed to an internal cause. Thus, amidst the mighty revolutions which have swept innumerable races of organized beings from the earth, which have elevated plains, and buried mountains in the ocean,—the rotation of the earth, and the position of the axis on its surface, have undergone but slight variations."

BOTANY.

12. *Indian Coffee.*—In a communication from India to Dr Traill, it is stated that a Dr Strong has succeeded in raising a marketable coffee, in some quantity, at Russypugla, about five miles from Calcutta. The attempt failed in the hands of Dr Wallich, and those of Messrs Palmer and Company, who tried it on a large scale. The secret of Dr Strong's success is in cultivating his plants *in the sun, not in the shade.* He states, when cultivated in the shade of other trees, as has been recommended by some, the roots and branches are more scanty than when they grow in the *full sun.* We expect, in our next Number, to report as to the qualities of the coffee, from experiments made in this country. Dr Strong says that if they can succeed in the formation of artesian wells, the cultivation may be extensively carried on in his neighbourhood, and with great prospect of its becoming a grand article of commerce. Should it equal, or be near in quality, to Mocha coffee, it will undoubtedly be of vast importance to Bengal.

STATISTICS.

13. *Academy of St Petersburg.*—The *sixth series* of the Memoirs of this Academy commences at the centenary celebration of this learned body, held in 1826. Up to this date, the complete collection of its volumes comprehends five series, each of

which is marked by a change of title. From the foundation of the Academy in 1726, to 1823, the Latin language was the medium of communication. The first series, called *Commentaries* (*Commentarii*), extended from 1726 to 1747; that is, from the inauguration of the academy, by the Empress Catherine I., until the Empress Elizabeth affected some new regulations. This series is in fourteen volumes. From 1747 to 1776, there are twenty-one volumes of *Novi Commentarii.* The celebration of the semi-secular jubilee established a new epoch, from which the publications are called *Acta.* Twelve volumes of these bring the labours of the Academy to the year 1783, a memorable year, in which the academy was placed under the direction of the Princess Daschkoff, for in Russia there is no Salic law, even in the government of letters and science. Under the new *Directeur* (such was the title given to this lady by the Imperial ukase, which invested her with the direction of the Academy), fifteen volumes of *Nova Acta* terminate the publication in Latin The year 1803 was an important period to the Academy; the Emperor Alexander gave it new laws, and the French language was substituted for the Latin. But the period was unfavourable to academic labours; so that, from 1803 to 1826, but eleven volumes appeared, forming the *fifth series*, under the title *Memoires.* Lastly, a mode of publication more useful than that of entire volumes, viz. that of parts or livraisons, has been adopted.

List of Patents granted in England, from 31*st August to* 16*th September* 1831.

1831.

Aug. 31. To J. J. Jacquier, London, for "improvements in the machinery for making paper."

Sept. 5. To H. G. Dyar, London, for "an improvement in tunnelling, or method of executing subterraneous excavations."

5. To G. Forrester, Liverpool, engineer, for "certain improvements in wheels for carriages and machinery, which improvements are applicable to other purposes."

6. To W. Bickford, Tuckingwill, Cornwall, for his invention of "an instrument for igniting gunpowder, when used in the operation of blasting rocks and in mining."

9. To J. Neville, Surrey, engineer, for "his improved apparatus for clarifying water and other fluids."

16. To G. H. Palmer, London, civil engineer, for "certain improvements in the steam engines, boiler, and apparatus, or machinery connected therewith, applicable to propelling vessels, carriages, and other purposes."

List of Patents granted in Scotland from 3d April to 4th September 1832.

1832.

April 3. To James Philp, damask manufacturer, Dunfermline, for an invention of "an improvement in the manufacture of diaper and damask table-linen."

4. To Alexander Beattie Shankland, London, for an invention, communicated to him by a foreigner, of "a new method of spinning flax and hemp by means of machinery."

6. To George Lowe of Bricklane, in the county of Middlesex, civil engineer, for an invention of "an improvement or improvements in and connected with the manufacture of gas for illumination."

17. To Alexander Brown of Liverpool, merchant, and Herman Hendricks of Passy, near Paris, for an invention of "an improved method or methods of manufacturing the prussiates of potash and soda, and the prussiates of iron, also for the construction of certain apparatus, vessels or machinery to be used in the said manufacture, and a new or improved method or methods of employing the said prussiate of iron (or other prussiates of iron) as a substitute for indigo in dying all sorts of wools, and whether in the fleece, skein, spun or woven into cloth, stuffs, or otherwise, also in dying silks, cottons, linens, and in fact all other sorts and descriptions of textile or other substances fit for the purposes of receiving colours of a blue, blue-black, black, greens, bronze, or any other colours for which indigo has hitherto been used, either as a ground-work or auxiliary, and also for an improved arrangement of certain utensils and machinery to be used in the said dying processes."

17. To John Samuel Dawes, of Bromford, in the parish of West Bromwich, in the county of Stafford, iron-master, for an invention of "certain improvements in the manufacture of iron."

30. To John Potts, Richard Oliver, and William Wainwright Potts, all of New Mills, in the county of Derby, engravers to calico-printers, and co-partners, for an invention of "an improved method or process of obtaining impressions from engravings in various colours, and applying the same to earthenware, porcelain, china, glass, and other similar substances."

May 2. To Robert Montgomery, residing in the town of Johnston, in the county of Renfrew, in Scotland, for an invention of a "machine for a new mode of spinning cotton, silk, flax, and other fibrous substances." Communicated to him by a certain foreigner.

5. To John Heathcoat, Esq. of Tiverton, in the county of Devon, for an invention of "certain new or improved methods of draining and cultivating land, and new or improved machinery and apparatus applicable thereto, which machinery and apparatus may be applied to divers other useful purposes."

May 14. To **George Goodlet**, residing in Leith, proprietor of the London, Leith, and Edinburgh Steam-Mills, for an invention of "a new method of preparing rough meal from ground wheat, or other grains, previous to their being dressed for flour; also rough meal from ground barley, malt, or other grain, previous to their being put in the mash-tun for brewing or distilling."

21 To **Bennet Woodcroft**, of Manchester, printer, for an invention of "certain improvements in the construction of a revolving spiral paddle for propelling boats and other vessels on water."

22. To **Thomas Brunton**, London, and **Thomas Fuller**, London, civil-engineer, for an invention of "an improvement or improvements in certain mechanical apparatus applicable to the raising of water or other fluids, and also a new or improved mode of effecting the same object."

31. To **William Drake**, of Bedminster, near the city of Bristol, tanner, for an invention of "an improvement or improvements in tanning hides and skins."

To **John Jellicorse**, of Stansfeld Mill, in the county of York, spinner, for an invention of "certain improvements in spinning machinery."

June 2. To **Joshua Bates**, for an invention of "certain improvements in machinery, or apparatus for roving, twisting, or spinning cotton, silk, wool, hemp, flax, or other fibrous substances."

8. To **John Joyce**, of John Street, London, for an invention "of a certain improvement, or certain improvements, in machinery for making nails of iron, copper, and other metals."

22. To **Frederick Steiner** of Church, near Blackburn, in the county palatine of Lancaster, manufacturing chemist and Turkey-red dyer, "for a certain process or processes, by which spent madders, or madders that have been previously used, can be made to yield a great quantity of colouring matter, and for dyeing with the same of various colours all descriptions of cotton, linen, wool, silk, or any mixture of them, and also for improving for dyeing madders that have not been previously used."

July 2. To **Hugh Bolton** of Staples, in the parish of Bolton-le-Moors, in the county of Lancaster, carder, for an invention of "an improvement in machinery used for carding cotton and other fibrous materials."

21. To **Alexander Beattie Shankland**, London, Esq. for "a new method of spinning wool."

28. To **John Holt** the younger of Whitby, ropemaker, for an invention of "a mode or process for preparing and manufacturing certain fibrous substances."

Aug. 1. To **John Howard Kyan**, London, for an invention of "a new mode of preserving certain vegetable substances from decay."

Sept. 4. To **William Daubeny Holmes**, London, engineer, for an invention of "a new method of heating houses and other buildings, and of applying heat to various manufactures and other purposes."

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PLATE I.

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Fig. 8.

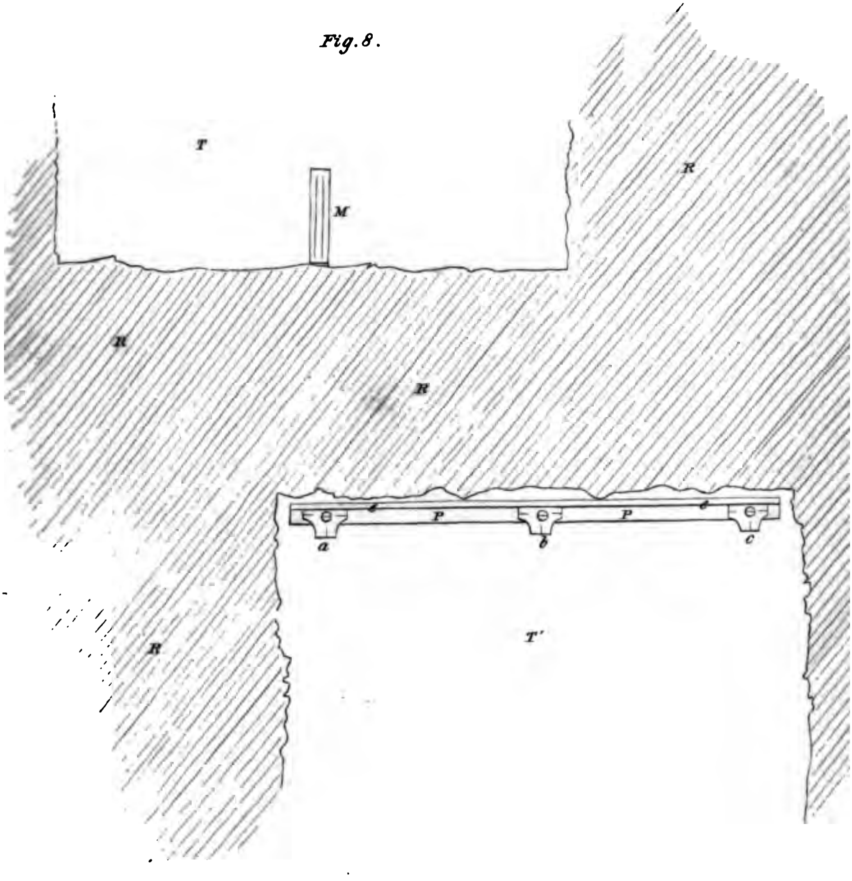
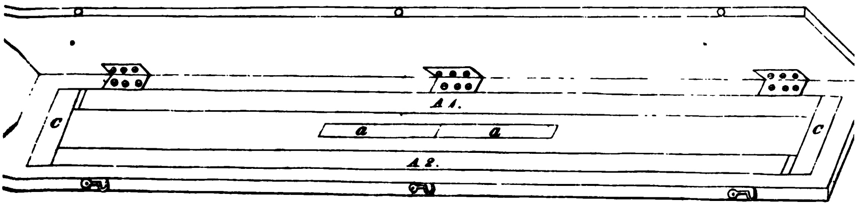


Fig. 6.

Case of Magnets.



E. Mitchell sculpt.

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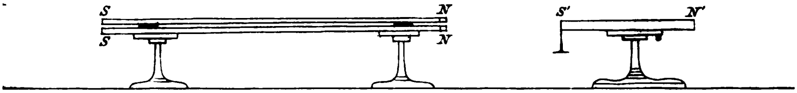

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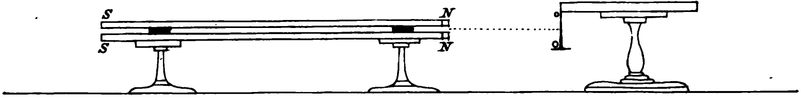

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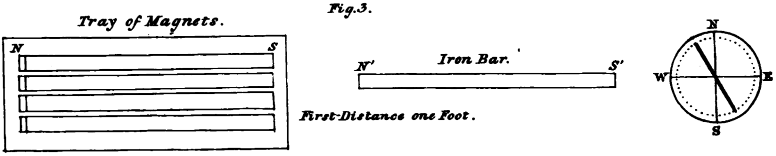

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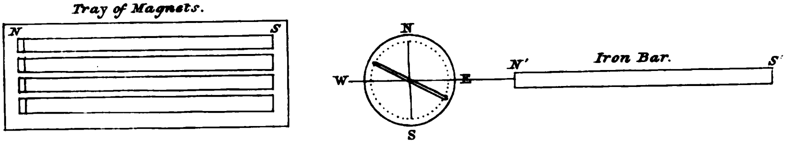

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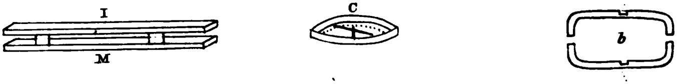

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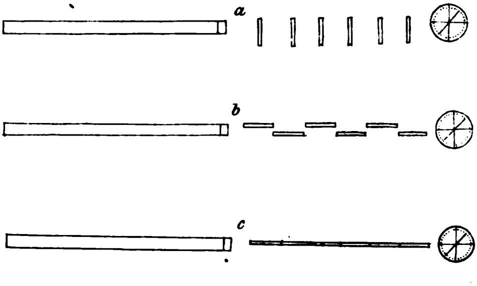

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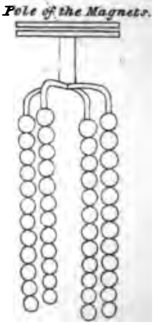
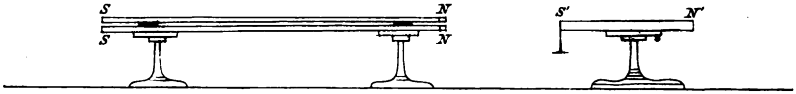


PLATE II.

Fig. 1.



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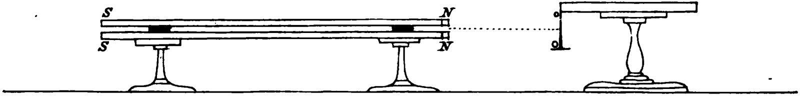

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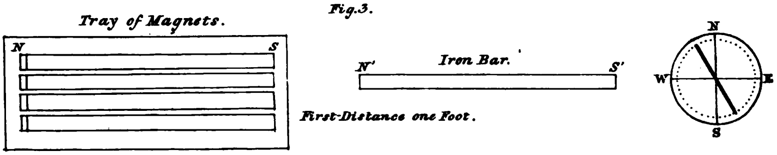

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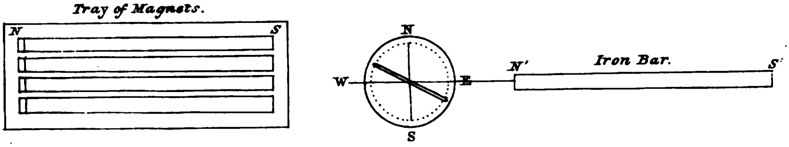

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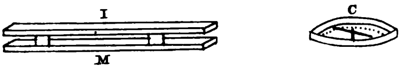

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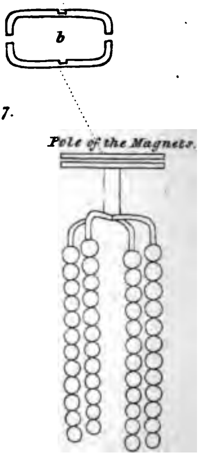

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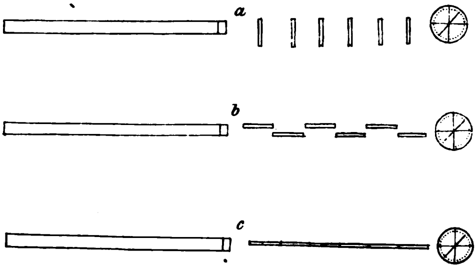

By del.

E. Mitchell.